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EDITOR:
EDWARD S. DUNSTER, M. D.

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Original Communications.

Art. I.—*On the "Sedative" Action of Calomel in Disease.*

By Frederic D. Lente, M. D., Cold Spring, N. Y.

"It may seem that any remarks, at the present day, on the principle or method of using *calomel*, must be trite, and altogether needless." This is the opening sentence of Sir Henry Holland's essay on "Mercurial Medicines," written thirty years ago. The same idea probably occurred to many of my present audience on hearing the subject of this essay announced. Those of you who have had the pleasure of reading the valuable essay of this distinguished writer, have, no doubt, risen from its perusal with a very different opinion. Although I cannot expect to afford you the pleasure and profit for which one may always confidently look in the discussion of any subject, however trite, by so able and classical a writer, yet I trust you will all agree that we have something still to learn of the mode of action, and especially the various applications, of this valuable but much-abused remedy; and possibly some

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[1] Read before the Dutchess County Medical Society, at Poughkeepsie, January 12, 1870.

of you may agree with me in the belief that we, as yet, know little or nothing of some of its most remarkable capabilities, of its most valuable adaptations to the arrest or relief of some of the most fatal affections of the system. I beg leave to apologize to the Society, at the outset, for the hurried and incomplete manner in which I have been compelled to handle this important topic, in consequence of the imperative demands and constant interruptions of general practice, which all of you will, no doubt, appreciate. On this account, I should not, at this time, have presented the subject for your consideration, but, having waited so long in the vain hope of a better opportunity, I have concluded to throw myself upon your indulgence.

The writer does not propose to advance any absolutely novel or startling ideas concerning the use of mercury, or *calomel*, which is taken as its great representative, and by far its most valuable combination, but rather to recall your attention to some of the oldest methods of its administration, never generally appreciated, and long ago abandoned by the greater portion of the profession.[1] I allude to the employment of the remedy in large, or, as the old term is, "heroic" doses; a scruple to half a drachm. It is astonishing that, in the case of a drug so universally employed by the profession, for so many centuries, and concerning which so much has been written by a host of the most able writers on medical science, there should be no settled principle of action established with regard to it, no definite rules for its application: indeed, that we should have to go back to the older writers to discover the most important indications which it is capable of fulfilling.[2]

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[1] Waring asserts, in his remarkable work, that "the *scruple doses*, advocated by Johnson, Annesley, and others, have been entirely abandoned." This is rather too sweeping an assertion. But many, who have, of late years, used them successfully, have shrunk apparently from advocating them publicly.

[2] "At the present day" (the first quarter of the present century), says Annesley, "Sketches of the Diseases of India," p. 367, "the opinions of practitioners, both in Europe and in India, are as various regarding the use or abuse of calomel, and the modes of exhibiting it, as at any former period."

Of late authors, Billing ("First Principles of Medicine") contends that it produces contraction of the capillaries, and thus relieves irritation and

The use of these large doses dates back at least as far as the middle of the seventeenth century, and the authors of that period, as Horstius, Sylvius, Juncker, Geoffroy, etc., exhibited these doses neither indiscriminately nor aimlessly; for they well understood, according to Annesley, the effect of smaller doses in producing ptyalism, and gave the larger doses for its far more important action. Subsequently, a dread of these potent doses spread itself over the minds of the profession, and the smaller doses were again substituted. Thus ptyalism was induced before any other manifest effect on the system was observed; and finally, this manifestation of its absorption into, and contamination of the blood, came to be generally recognized as necessary, in fact, as the *means* of ridding the system of the prevailing disease; a most unfortunate and fatal deduction. This timidity in the application of the remedy has proved to be the principal cause of its great abuse in subsequent times. Later, a revival in the use of calomel occurred, and we must attribute this to James Johnson.[1] His attention seems first to have been attracted to it accidentally, and painfully too. The history of his own case is so pertinent and instructive, that I venture to give the concluding paragraphs in his own words; premising that he contracted the disease (dysentery) while hunting on the banks of the Ganges; that his medical attendants put him upon the usual treatment (calomel in small doses, and combined with opium, and mercurial inunction), and continued this for a couple of days; that he constantly grew worse. He says that, at this stage—

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inflammation; while Headland ("Action of Medicines") flatly contradicts this assertion.

But it is scarcely necessary to quote further proof of what has been asserted, as all must be assured of the unfortunate diversity of opinion regarding this subject.

[1] Stillé, in his work on "Therapeutics and Materia Medica," attributes this (doubtful, as he considers it) honor to Annesley. But he is as much in error in this, as in his ideas of the objects and principles which guided this writer in his use of the remedy. Almost all, indeed, of our later writers on these branches of medical science, seem to exhibit this same want of appreciation of the objects aimed at, or attained, by those authorities who have had the tact and prudence to administer calomel in "heroic" doses, but in a proper manner, and in suitable cases and stages of disease.

> “The surgeon endeavored to cheer me with the hope of ptyalism, which, he assured me, would alleviate my sufferings. I had then no local experience in the complaint myself. As the night advanced, all the symptoms became aggravated, and I was convinced that a fatal termination must ensue, unless a speedy relief could be procured. I had no other hope but in ptyalism; for my medical friend held out no other prospect. I sent for my assistant, and desired him to give me a scruple of calomel, which I instantly swallowed, and found that it produced no additional uneasiness; on the contrary, I fancied it rather lulled the tormina. But my sufferings were great—my debility was increasing rapidly, and I quite despaired of recovery! Indeed, I looked forward with impatience to a final release! At four o'clock in the morning, I repeated the dose of calomel, and at eight o'clock [or between sixty and seventy hours from the attack] I fell, for the first time, into a sound and refreshing sleep, which lasted till near midnight, when I awoke. It was some minutes before I could bring myself to a perfect recollection of my situation prior to this repose; but I feared it was still a dream, for I felt no pain whatever! My skin was covered with a warm moisture, and I lay for some considerable time without moving a voluntary muscle, doubtful whether my feelings and senses did not deceive me. I now felt an uneasiness in my bowels and a call to stool. Alas, thought I, my miseries are not yet over! I wrapped myself up, to prevent a chill, and was most agreeably surprised to find, that with little or no griping, I passed a copious, feculent, bilious stool, succeeded by such agreeable sensations—acquisition of strength, and elevation of spirits—that I ejaculated aloud the most sincere and heart-felt tribute of gratitude to Heaven for my deliverance.”

Now, this graphic account introduces us to what I wish particularly to direct your attention to, the *sedative* power of calomel in large doses; for its action in this case could not probably have been supplemented by any other remedy known then, or subsequently discovered, as I have ascertained by actual experience. After some years, another reaction in the history of the remedy occurred; and, in our modern treatises, these “heroic” doses are, with very few exceptions, only mentioned to be condemned.

The writer's first experience with sedative doses was in epidemic dysentery. An account of this was published in the *New York Journal of Medicine* for March, 1856, and I quote a few lines from it: “Large doses of opium (three to four grs.), and large anodyne enemata (ʒ i to ʒ ij), failed to bring any relief to the distressing tormina and tenesmus. The efficacy of scruple-doses of calomel, so highly recommended by Johnson, Annesley, and others, and recently indorsed by the high authority of Prof. Dickson, of Charleston, was then tried, and with signal success. Its action was usually this: it was generally administered in the early stage of the disease" (only in the severe cases), "very often as the first prescription" (most of the patients having tried various remedies before I was called). "The patient would be suffering the most intense cutting pain across the abdomen, often accompanied by considerable tenderness on pressure, distressing tenesmus, and passing blood or bloody mucus every ten or fifteen minutes, and earnestly desiring some immediate relief. One scruple of calomel was then given; within an hour, generally (sometimes in half an hour, once in fifteen minutes), relief, sometimes complete, would be obtained." For five or six hours after, frequently eight to ten hours, there would be no discharge from the bowels, and very little uneasiness of any kind. In a few cases, the bowels were constipated for twelve hours or more, requiring a dose of castor-oil to move them. Generally, after two or three hours' relief, the patient would have two or three loose bilious evacuations, brownish or greenish, sometimes attended by some pain and griping, sometimes not. In not a few cases, the distressing symptoms did not recur at all, and convalescence commenced. In a majority of cases, however, in from twenty-four to thirty-six hours after the operation of the calomel, the dysenteric symptoms returned, though in a decidedly mitigated form, there seldom being any severe pain or griping (when a second dose of calomel was given; but generally the case was completed by a drachm or two of oil, or a few small doses of opium or Dover's powder). In but very few of the cases did the mercury produce any ptyalism, or any decided affection of the gums, and in no case did it produce any severe mercurialization. The dose was given in forty-seven cases, and seldom repeated, showing a marked difference in obstinacy, in epidemic dysentery here, and that which prevails in India. There, also, ptyalism was generally induced before the symptoms entirely yielded. Since the publication of this paper on dysentery, I have discovered, from a larger acquaintance with the use of calomel in full doses, that *half-drachm doses* are, for adults, *safer* (that is, less apt to irritate, and not more apt to salivate) than the scruple-doses. In fact, it is well always to bear in mind, when using the "calomel treatment," that there is more danger in giving too little than too much, or (to speak definitely) in giving less than twenty grains, than a little over thirty.[1] I have never thought it justifiable to attempt to ascertain to what extent the dose may be increased without doing injury, since a larger dose than half a drachm I have not yet found necessary. But writers of high repute have exhibited forty, fifty, and sixty grains, with the best effect in desperate cases. Prof. W. Parker once mentioned to me a case which occurred under his observation many years ago, in which a delicate child, almost hopelessly ill with "membranous croup," took, in three or four days, *half an ounce* of calomel in scruple-doses, and recovered perfectly. There does not appear to be such a thing as a *poisonous* dose of calomel. Yet, according to Parrish ("Pharmacy," article *Calomel*), ptyalism may be produced by one grain divided into twenty-four doses, and might thus do more injury than forty grains in one or two doses, so singular are the reactions of this powerful agent on the system.

Calomel exerts its sedative power almost if not quite as remarkably and successfully in "membranous croup" as in dysentery. Indeed, judging from my own somewhat limited experience with it in this disease, I may ascribe to it, in proper doses, and aided by such appliances as will keep at bay the urgent danger while the calomel is acting, the powers of a *specific*. Should this prove, on sufficient trial (which it has never had), to be the case, what a boon is it![2] Before this treatment was recommended to me by a medical friend in New York, one of the most distinguished teachers there, the fatality among my cases of "croup" was such that I trembled when called to a case. And this has been the experience of all those who have had a large acquaintance with the diseases of children, and who have been careful to distinguish the true from the spasmodic variety. Dr. Ware, a high authority, states that nineteen out of twenty die. When *tracheotomy* has been resorted to sufficiently early, as it is not apt to be, a larger proportion has been saved. But, if we have not, in sedative doses of calomel, a remedy for membranous croup, we certainly have in no other drug or combination of drugs.[1] Various articles, mainly of the depressant, nauseating, or emetic class, have been lauded, as particularly successful in croup, by different practitioners from time to time. They generally only palliate distress, however, but lengthen the agony, as a fatal result pretty surely ensues. Unfortunately, it is not an easy matter to distinguish true from false croup, until quite late in the disease. I am speaking from my own experience, as most authorities, except Vogel, would have their readers believe it comparatively easy, even at the outset of the disease. In true croup it is rare to see the membrane, at any stage of the disease, *in the fauces* (except in the *diphtheritic* form, when it is almost always visible), and not very common to see it in a distinct form in the expectoration; and if not seen, the true nature of the case cannot be surely diagnosticated but by its progress and the result of the treatment employed.[2] If a case

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[1] Johnson speaks of a scruple as the maximum dose, and cautions against an increase. My own trials of the remedy, as has been remarked, led me to a different conclusion. And Prof. Dickson says of this caution: "We smile at this attempt at precision. It is not permitted, at the present day, to talk of a definite limit in the exhibition of any article of the materia medica, or to venture to predict the effect of whatever additions to the established doses of ancient formulæ."—*Dickson's Practice, Art. Dysentery.*

[2] It is somewhat singular that the writer should have met with quite a number of cases annually of true croup during the first ten years of practice, and that during the last ten years it has become quite rare, although the infant population has increased. During the past five years, he has not averaged more than one case per year, both in his own practice and that of his assist-

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ant, and this accounts for his limited opportunity for testing the calomel treatment properly.

[1] Among the latest writers on diseases of children, Dr. J. Lewis Smith says: "It is uncertain whether it does exert any controlling influence upon the progress of croup." He does not state whether he has had any experience with the "heroic" doses.

[2] "The reason for these diverse statements," says Vogel, in his recent work on "Diseases of Children," "is found in the diversity of the anatomico-pathological processes. In the one case, croup is produced simply by a plastic exudation within the *larynx*, which does not generally extend above the *epiglottis*. In the other by diphtheritis, which almost always occurs simultaneously upon the tonsils. In France, especially Paris, it is rare to find a child with croup, and without diphtheritic patches on the tonsils;

commences rather moderately, the symptoms continuing, though in a mitigated degree, through the day, finally becoming alarming; and if, after a successive and thorough trial of the usual remedies, external and internal, including, of course, as frequent emesis as the patient's strength will bear, the symptoms do not yield, but, on the contrary, increase during day and night, with perhaps short intervals of temporary relief, we had better infer that we have a case of "croup," and abandon the emetic and depressant treatment as worse than useless. The writer has never made use of the "calomel treatment" in any case of this disease until a fair trial has been made with the ordinary remedies. Indeed, in most of the cases so managed, he has been called in consultation by other physicians *in consequence* of the failure of treatment. His first experience was in the case of a boy about five years of age, in a family where a number of children had died from year to year, some of them from croupal affections; not one had ever recovered from the latter. My then assistant, Dr. Hardaway, formerly house-physician at Bellevue, an accomplished physician, had charge of the case, and, having exhausted all his ingenuity and resources, and apprehending a fatal result, summoned me to the case. We immediately commenced giving scruple-doses of calomel. It was bedtime when the first dose was given; and several similar doses were left with the mother, with directions to administer one in syrup whenever the paroxysms (dyspnœa) should become alarming. In the morning we found the child very much better; cough becoming loose, and inspiration quite easy. The mother stated that, within half an hour from the time the first dose was given, "the child fell into a *quiet sleep, and breathed easily*;" that in a couple of hours he aroused, and the paroxysm became quite urgent;

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while, in Munich and vicinity," he states that "the reverse is the case." This accounts for the fact that no less an authority than Niemeyer, in his late work on "Therapeutics," diagnoses *croup* by white patches early appearing on the tonsils, evidently diphtheritic croup.

Dr. Edward Ellis, of London, in a recent work on "Diseases of Children," commences his description of "croup" with this sentence: "This disease is very alarming in its symptoms, contagious, and very fatal." He would hardly use the term *contagious* with reference to ordinary membranous croup.

whereupon she gave a powder, with a similar result. And so she found it necessary to continue until he had taken four doses (eighty grains) in ten hours. Suffice it to say, that he was threatened slightly once during the day, and a scruple more was given as a precaution; and, on the following day, he was quite well, except a slight affection of the gums, which yielded within forty-eight hours to chlorate of potash. The only cases of which I kept full notes are the following; for it must be confessed that, in my first cases, I had so little expectation of any such results, notwithstanding the responsible authorities by whom the treatment was recommended, that it did not occur to me to keep notes. The case of J. G., although long, is given in full, because it illustrates the proper method of employing what the writer considers the two most important means of cure in the disease, means which are so seldom properly handled; and the necessity of the physician's superintending their details personally, and carrying them out thoroughly. Another precisely similar case occurred during the same winter, and singularly enough, in the next house, in a stout child of about the same age, who was treated in a similar manner, except that the steam was not so perseveringly applied, and that *fumigation* with calomel seemed to aid its internal administration. This case also recovered. It is interesting to mention that he had been so thoroughly and constantly vomited and purged by the alarmed parents, when I could not be found, as to render it very difficult for him to retain even calomel on the stomach, which, when it was retained, checked promptly both the emesis and cartharsis without recourse to any opiate:

J. G., a stout, healthy boy, three years old, became fretful and ailing on January 16, 1867. On the 17th, had a cough, which at eight became "croupy," and the respiration also. His mother administered paregoric and syrup of ipecac., with the effect of vomiting him two or three times, and with some relief to the symptoms. 18th, A. M. No better. Had what his mother called a "spasm" from difficulty of breathing, and was vomited again. Dr. B. A. Segur, formerly house-physician of Bellevue, now of Brooklyn, saw the case in the afternoon, in my absence from home. He had had a recurrence of the *dyspnœa;* and Dr. S. advised a continuance of the medicine, with the addition of the inhalation of the vapor of boiling water. He also gave a mercurial cathartic (5 grs. of calomel).

19th, A. M. Doctor hurriedly summoned, and found him much worse, although he had been repeatedly vomited. The vapor had been applied rather inefficiently. The cathartic had produced two very dark evacuations. The doctor now remained with the patient, and superintended the steam arrangements himself, and soon had the room *filled* with the vapor, and conducted directly toward the child's face by a thick paper tube. In order to generate large quantities of vapor, large cobble-stones were heated to redness in a range, and put into a tub with some three inches or so of water in it. These were changed every few minutes day and night. The temperature of the room was kept at 73° to 74° F.; the thermometer outside the house ranging from 3° to 10°. The heat of the room was maintained almost entirely by the steam, only a very little fire being kept up. In consequence of the extreme cold externally, the steam rapidly condensed, and ran down the walls in streams, and, after twenty-four hours, dropped from the ceiling, rendering it occasionally necessary to wipe the latter dry. All the bed-clothes and curtains were thoroughly dampened. Those about the child were occasionally changed as they became very damp.[1] During the day, had the croupy cough and respiration, but was easier, and laid more quietly in his crib. Toward evening the symptoms grew more threatening. Inspiration interrupted. At this juncture, the writer returned and took charge of the case. 6.10 P. M. Determined, Dr. S. concurring, to try the "calomel treatment." Gave a scruple. 8.45 P. M. Half an hour after dose, breathed somewhat easier; but has been restless; has slept almost none for forty-eight hours. Gave ℞ calomel ℈i, pulv. Tullii gr. iv. ♏.

January 20th, 5 o'clock, A. M. He slept almost all night, except when aroused to take beef-tea, which has been given regularly at stated intervals. Has just had one dark, slimy evacuation. Is sweating profusely.

20th, 5 o'clock, P. M. During the early part of the afternoon was quite playful, voice better; respiration still croupy, but with no painful effort. Sweating profusely now, dull, feeble, languid; pulse feeble and irregular. Respiration becoming *hurried.* ℞ spts. vini gall. ʒ i q. 2. h. Also fumigated with calomel ʒ ss. This produced intense irritation of the air-passages, and a most incessant and harassing cough, which failed to detach any membrane or to relieve the *dyspnœa.* Pulse 130 to 140, but stronger from the stimulant. Finally, was compelled to prescribe Tully's powder.

January 21st, 1 o'clock, A. M. Is very restless. Skin now dry, respiration *hurried,* and stridulous; pulse less frequent. Deglutition is so difficult at times, that the nourishment and stimulant are now given *per rectum.* ℞ calomel ℈i, p. Tullii gr. iij. ♏. Has had two dark, greenish evacuations within the last twelve hours. 7 o'clock, A. M. Has been quiet, and sleeping part of the time. Cough croupy still, but less harassing. Pulse still frequent. Skin dry. ℞ calomel ℈i. 9 o'clock, P. M. Half an hour after the last dose, symptoms began to improve. Seemed to get rid of some obstruction after coughing hard. Pulse better. Respiration becoming less hurried and easier; one dark, greenish-yellow evacuation. 2 o'clock, P. M. Improving. Stop stimulant. 6 o'clock, very lively; voice almost natural. Air enters lungs much more freely than it has done at any time. (The vapor has been persistently kept up day and night, it being at times difficult to distinguish between persons across the room, by candle-light. During the last few hours, it has been allowed to decline a little.)

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[1] There is no danger from dampness, provided a high temperature is kept up.

January 22d, 1 o'clock, A. M. For two hours, has been restless, but not apparently suffering. Respiration slightly dry, and croupy. Skin pleasant. Has swallowed his beef-tea with less resistance during the night. ℞ calomel ℈i, p. Tullii gr. ijss. ♏. 22d, 7 o'clock, A. M. Went to sleep an hour or so after the dose, and has slept until an hour ago. Breathes easily, but cough is still slightly croupy. Had one evacuation like the last this morning. Skin more moist; pulse fair. Brandy ℥ ss. q. 2. h., as he seems rather languid. 6 o'clock, P. M. Rapidly improving. Respiration almost natural. To stop vapor. Takes some solid food with relish. To take ℞ pot. chlorat. gr. vi. quin. sulph. gr. ij ♏ ter die.

January 23d, a little restless and peevish, but looks well. Respiration perfectly natural. Pulse rather feeble. Two dark-green evacuations. From this time he convalesced rapidly, and in a few days was in his usual health, and has never been sick since.

F. D., aged five years, previous health good, except an attack of "whooping-cough," of several weeks' duration, of a mild character. Had been suffering from a croupy cough for two or three days, the result of a wetting, when, at 3 o'clock, A. M., January 28, 1869, his mother was alarmed by a sudden increase of the *dyspnœa*, and commenced dosing him vigorously with syrup of ipacac. and paregoric. This was kept up assiduously until 9 o'clock, P. M., of the same day, when I was called. The medicine had vomited him several times; the anodyne had induced considerable drowsiness, and he had slept at intervals, but the symptoms grew worse. There was nothing unusual about the fauces, and but little fever. Pulse feeble. Had kept little or no nourishment on the stomach through the day. Deeming it useless to continue the nauseating remedies any longer, it was resolved to try the specific treatment for croup, and calomel ℈i was at once given, with directions to continue every two hours *until the symptoms abated*, or until the characteristic evacuations were observed. It was also directed that the crib should be surrounded by a thick canopy, under which steam should be constantly introduced by a generator provided for that purpose.

January 29th, 7 o'clock, A. M. Has had three doses of calomel, and the vapor was kept up until near daylight, when, as the symptoms appeared much less urgent, it was omitted. About a quarter of an hour after the first dose of calomel, the child vomited, as he had been doing for the past eighteen hours, soon fell asleep, and breathed easier than he had done for twelve hours. He subsequently had a recurrence of the symptoms, and a second powder was given at 3 o'clock, A. M., and again, on a further recurrence of the symptoms, at 5 A. M. Soon after this the improvement commenced. He is now breathing easily, and feels much better. Cough croupy, but loose. Still nauseated; one evacuation, formed, and dark green. To continue steam, and to give the calomel on a recurrence of paroxysms. Dr. Murdock, my assistant, now took charge of the case.

January 30th, Dr. M. reports that, by mistake, the mother gave a powder (calomel ℈i) at 9 o'clock, A. M., yesterday, and that the steam has been kept up until this morning; when, the cough being loose, and the croupy respiration having entirely disappeared, it was discontinued. The last dose was followed by loose, dark-green, and slimy evacuations, when the nausea immediately disappeared, and he called out for toast, which he took with relish, and he has had no nausea since. Pulse, which was quite feeble throughout the attack, is now much stronger. Took eighty grains within twelve hours.

May 30th, three months after attack. Has continued in perfect health ever since.

Membranous croup has been treated, by scruple-doses of calomel, by some of our most distinguished physicians and teachers of medicine in New York City, for nearly twenty years, and is still relied upon by some, but to what extent it is difficult to say, for any published allusion to such treatment is very rare; though, in private conversation, these gentlemen speak of the treatment as more to be depended on than any other. In fact, the prejudice against calomel, especially in large doses, is still so great, both among the profession and the laity, that few men care publicly to proclaim the extent of their confidence in its curative power. The introduction of the calomel treatment in croup is ascribed, in New York, to Dr. Bay, of Albany.[1] But Hamilton, in his work on "Mercury," published in the early part of this century, says that American physicians have been using it in croup as the chief remedy, and that its use spread thence to England. I can find no mention made of large *doses*, but large *quantities* (one hundred grains and upward) were required to produce the effect.[2]

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[1] Prof. Potter, of Batimore, was also in the habit of using it in "enormous" doses, and he claims with "uniform" success.

[2] The American editor of Hamilton, Dr. Ives, remarks, in a foot-note: "I know it is said, by Dr. Chapman, that some of the respectable practitioners, both of this country and of Europe, *trust exclusively to calomel* in croup; but, if there be any who are thus presumptuous in their practice in this country, they have not had the courage to publish it to the world."

It is interesting and instructive to quote what an author of such renown, and who has written the most systematic treatise extant, against the use of calomel in disease generally, has to say of his experience in the calomel treatment of croup. Recognizing the futility of blood-letting, emetics, purgatives, blisters, etc., etc., and of the ordinary mode of using calomel, Prof. Hamilton was induced, by a medical friend, to give the American treatment, as he styles it, a trial. "At any rate, he can solemnly assert," he says, "that, according to all he has seen, no relief whatever has been afforded by that medicine, unless copious dark-green colored stools, like boiled spinach, have been discharged, and that it requires *large and repeated doses* of the medicine to produce that effect. For example, to a child of seven years old, one hundred and thirty-three grains were given within sixty hours." He further says: "In the only cases in which this medicine has failed under the author's direction (being in the proportion of *four* out of *fifty*) no evacuation, through the bowels, could be produced. . . In reasoning on this subject," he continues, "it is extremely difficult to explain, in the first place, the safety with which a hundred and thirty-three grains of calomel could be given in this climate (England), within sixty hours, to a child of seven years. Secondly, the relief which has invariably followed the discharge of the dark-green colored evacuations." The loss of only four cases out of fifty is the most remarkable success that I have seen recorded. These remarks of this author give the clew to the effectual employment of calomel in croup; that is, *to continue the remedy until permanent relief is obtained*, or until free evacuation *of the stools just described*. But the peculiar *sedative* influence, which we have witnessed from the scruple-doses, is not obtained from the small doses, however frequently repeated. He goes on to state that "considerable weakness remains after the crouping has ceased, partly from the violence of the symptoms, and partly from the operation of the calomel." He adds that "he has seen two cases where the patients sunk from the weakness which followed." But we must not allow our patients to sink. It has been seen, in the case of J. G., how suddenly weakness may supervene, and how promptly, also, it can be counteracted by timely stimulation. It is not fair, however, to ascribe this weakness to the remedy, except to a slight extent, since we observe no such debility ensue from similar doses in any but these fatal and prostrating diseases. In coping with so deadly an enemy as this, no judicious physician would omit to employ all the weapons at his disposal, in case of necessity, whatever his reliance on any special one; and, although the author of this paper believes that calomel alone, if not too long deferred, and boldly repeated when required by the symptoms, is capable of curing the disease, yet it has been seen that he does not, in any case, fail to use the most effective adjuvants; and he begs leave again to direct attention to what he considers by far the most important of these, and to insist on as bold a use of this as of the calomel, as the only means, not only of securing any benefit, but actually of avoiding injury. If no special apparatus is procurable, and the room be not large, it may be filled with vapor, as has been described in the case of J. G. But this can only be done where there is a range or very large stove, and a strong man always at hand, for days if necessary, and therefore not in the dwellings of the poorer classes. A very simple contrivance, which any tinman can furnish at a moderate expense, will answer admirably. It consists of a boiler of the size of a large tea-kettle, with a long shifting spout to conduct the vapor under the bed-clothes, or canopy, a tin stand for it, and a large tin spirit-lamp, holding about a quart, with three or four large wicks. This can also be used for giving a vapor-bath and a copious sweat in Bright's disease, etc., by putting hoops over the patient, as he lies in bed, and the clothes over these; or, if not too sick, he can sit on a chair with a blanket drawn around him closely, and secured about the neck. The first record of the efficient use of this agent in croup, which came under my notice, was a case published by my friend Moreau Morris, M. D., now connected with the Metropolitan Board of Health. It is very easy to separate the effect produced by this and other adjuvants in the above cases, from that produced by the calomel. The great benefit derived from the steam is the *present relief of dyspnœa*, and the prolongation of life, in obstinate cases, while the calomel is silently producing its curative effects. It is easy to see, by a study of these cases, that vapor alone will not cure.[1]

In *epidemic cholera* the effect of sedative doses of calomel is sometimes more marked than in any other disease. Since his attention has been directed to this subject, the writer has not had an opportunity of testing it in many cases personally. In the stage of violent cramps, he has found it advantageous to inject half a grain of morphine, hypodermically, and repeat, if necessary, until they are subdued, then to give calomel in half-drachm doses. Calomel has this advantage, in cholera and similar affections, that it is almost the only remedy which can be borne on the stomach. Mr. Jamieson, in his report to the British Government, on the disease as it appeared in Bengal in 1817–'19, says: "Although it cannot be affirmed that calomel possessed any especial power in checking the disorder, it was undoubtedly frequently useful in checking irritability; and was, perhaps, of *more certain sedative action* than any other medicine." The practice of giving scruple and larger doses of calomel in cholera was very general among tropical physicians; but they frequently premised copious bleeding, and often gave considerable *laudanum*. The most complete exhibit of the effect of calomel, *per se*, in cholera, is furnished by Dr. Vanderveer, of New York, in his report of the Franklin-Street Cholera Hospital opened during the epidemic of 1854, and under his charge, assisted by Dr. (now Prof.) C. A. Budd, and Dr. Richards.[2] In the first stages of confirmed cholera, he gave thirty-five to forty grains, and used hot applications. If the dose was rejected by the stomach, it was repeated in five minutes, "and so on until emesis ceased, or we saw some signs of the medicine, or judged a sufficient quantity had been retained to proceed at regular intervals with diminished doses. In the stage of *collapse*, we gave sixty grains, and if they vomited soon after, and the medicine could be detected in the vessel, we repeated it at once, and so on until the third or fourth dose was administered. Frequently, if vomiting did not ensue, one dose would be sufficient." He says, with reference to any fear of purgation: "I have never met with a case of cholera, treated as above stated, and hypercatharsis ensue; on the contrary, it has frequently occurred that patients, both in hospital and private practice, have been seized with violent vomiting, purging, and cramps, which had, from their own statement, been kept up every ten minutes for one, two, and three hours; and, after taking sixty grains of calomel, have not vomited or purged for six, ten, and twelve hours after; and, in two or three instances in the hospital, after waiting twelve hours, resorted to mild enema to open the bowels." He further says: "I have found calomel in large doses more sedative (than cathartic) in its effects, and that its cathartic action does not increase in proportion to the increase of the dose." As regards his results, nearly one-third of the cases were received into the hospital in a state described as "profound collapse," some of them pulseless at the wrist. Of these, nearly *one-third* were saved. Of those in "partial collapse" over one-third, and this under rather unfavorable surrounding circumstances.

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[1] Dr. West ("Diseases of Children") makes the remark that "the action of mercurials is far too slow to overtake a disease which tends so rapidly to a fatal issue," but evidently refers to the ordinary methods of using the remedy, and does not appear to place any reliance on the scruple-doses, or does not refer to them at all. For no other remedy, in this disease, acts so promptly.

[2] New York Journal of Medicine, September, 1869.

In violent *cholera morbus*, with excessive irritability of stomach, when, in fact, nothing else could be retained, I have given a scruple to half a drachm of calomel, with the effect of checking all the symptoms very promptly. I have had but little experience with the sedative doses in other diseases than those above enumerated, but am convinced that we have yet a good deal to learn of their value in various other complaints, especially in those attended by sudden congestion of the viscera. To a lady, who had had repeated attacks of diphtheritic tonsillitis, generally lasting several days, and attended by considerable debility, as they usually are, I gave twenty-five grains at the inception of one of her attacks; the next day, she was much relieved, and the attack was apparently aborted. Mrs. H. P., aged thirty-five, of rather delicate organization, was attacked, a couple of weeks since, with chilliness, violent pains "all over," sore-throat, and debility. The fauces were inflamed, the tonsils swollen, and showing patches of diphtheritic exudation. I administered thirty grains of calomel, and small doses of chlorate of potash were directed at intervals, more as a placebo. Dr. Murdock saw her, for me, the next morning, and found her very much relieved. She did not require another visit; and was neither purged nor salivated. It is known to many of the profession in New York that it was the practice of the late Dr. Cammann, one of the ablest diagnosticians and respected practitioners that we have had, to administer heroic doses of calomel (twenty, forty, fifty grains) for the purpose of aborting sudden and dangerous attacks of thoracic disease, and with success. In one instance, says his friend Dr. J. R. Leaming, in a recent letter to the writer, he gave a large dose to a patient with a tubercular cavity, and who had been suddenly attacked with extensive pneumonia on the diseased side, and was in a very critical condition. "In the morning," says Dr. L., "he was quite relieved, and the pneumonia aborted. The patient is yet alive, and Dr. C. has been dead these six years." Dr. Leaming's attention was, like that of Dr. Johnson, first called to the good effect of the calomel treatment by its result in his own case. I hope he will pardon me for giving his own words. "I was taken," he says, in a letter to the writer, "while at lectures, with a violent chill, and pain in the head and back, got to my room as soon as possible, and prevailed upon a student to give me twenty or twenty-five grains of calomel. That was on Thursday, and, on Sunday following, Prof. Dickson saw me and recognized typhus fever and also the happy effect of the calomel; but advised us not to follow out that treatment on our patients when we got into practice. I was at lectures again in two weeks." He states that he has used the same treatment since in "fever," when the *prodromata* were violent, and always beneficially.[1] In a paper on "Pleuritis," read before the Academy of Medicine recently, Dr. L. states that he uses the large dose, twenty, thirty, and forty grains, "not for its purgative, nor yet for its constitutional effect—the former is undesirable, the latter disastrous—but to produce a profound impression upon the great organic nervous system." This he relies upon, with the *juvantia*, to abort the inflammation, and "prevent serous effusion, and deposit of lymph, with their train of serious evils."

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[1] Dr. Leaming is not alone, even among the latest writers on therapeutics, in his opinion of calomel in low fevers. Dr. Sidney Ringer, in his recent work on "Therapeutics," says: "Typhoid fever, according to very high authorities, among whom may be mentioned Dr. Parkes, may be most beneficially influenced by small doses of calomel."

Let us now, as briefly as possible, review the opinions of authors, especially on tropical diseases, who have employed this remedy, with regard to its object, its *modus operandi*, and its mode of administration, and endeavor to reconcile the discrepancy of results, which has so greatly tended to bring this system into disfavor within the last decade. It will be observed that these writers now and then use the term *sedative*, and that Johnson and Annesley incidentally distinctly claim this action, as does Dr. Vanderveer. But all these writers seemed to rely more on its alterative power, and especially its peculiar action on the mucous membrane of the intestines and on the liver; most of them aiming at ptyalism, mainly, however, as an index of its proper action on the system. Dr. Leaming claims for it a distinctly sedative power, and a power of producing a prompt, a profound, and a favorable impression on the sympathetic system of nerves, tending to abort, or to render dangerless, those violent forms of disease, some of which have been alluded to in this paper, and of whose behavior, under this treatment, we have furnished illustrations. It has been with these views, and in such cases usually, that the writer has employed this treatment for a number of years. Headland, one of our highest authorities on the action of remedies, positively denies any sedative power to these doses, and rejects them altogether.[1] Stillé, and most others of the later authorities, agree with Headland's views.

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[1] This author, while disapproving of the large doses, not as dangerous, but as simply useless, because, as he claims, not absorbed, still evinces great confidence in the remedy in small doses. "The more we know of its real actions," he says, "of the mystic processes of its absorption and operation on the system, and of the comparative physiological tendencies of the various forms in which it is administered, the better shall we be able to wield it with skill and effect. In a line of investigation, and of patient experiment on disease and remedy, lies our best and wisest course." He evidently permits his *theory* (of non-absorption) to prevent a personal trial of sedative doses.

The fact has already been alluded to that, in their management of dysentery, cholera, etc., the tropical physicians were in the habit of premising bleeding, and frequently of quieting irritability by combining a moderate amount of laudanum with the use of calomel; and many might infer that the sedative effect attributed to this remedy was due mainly to those powerful auxiliaries. It is very probable that the venesection caused a more prompt absorption of the remedies subsequently administered, but it is also very probable that it was at the cost of rendering the disease more tedious, and rather of impeding the ultimate favorable action of the calomel; at all events, the experience of the last twenty-five years has proved this in temperate climates. Laudanum, and sometimes solid opium, was given occasionally, at the inception of an attack, to quiet urgent symptoms; but it was generally avoided as interfering with the proper action of the calomel. I have not unfrequently observed that, in cases of obstinate fecal impaction of the *cæcum*, when various laxatives, enemata, etc., have been continuously used without effect, until the pain and increasing exhaustion of the patient forced their discontinuance, a full dose of morphine would give a few hours' relief, and perhaps sleep, to be followed by a copious discharge from the bowels. Yet the cure is not attributed to the anodyne. Thus, in the case of J. G., small doses of opium were given for a special purpose (the only case in which the writer has been compelled to associate it with calomel in croup), but no one can, after a careful scrutiny of that case, attribute more than a very subordinate, though an important agency, in the cure, as important perhaps as the stimulant. In my cases of dysentery, the calomel was uninfluenced, as a very general rule, either by bleeding or by opium, neither was salivation considered desirable, yet my results were precisely similar to those obtained by the tropical writers. So were Dr. Vanderveer's.[1]

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[1] It is worthy of note that it is claimed by Cunningham and some others that hepatitis and hepatic abscess are much less common *sequelæ* of dysentery when the latter is treated by the large doses of calomel. He states that, although the disease prevailed to a greater extent in his ship (Sceptre) than in any other in India, yet not a single instance of hepatitis, supervening on this disease, occurred. "In the Albion and Russell, where much less calomel was used, liver-complaints were very prevalent."

Although, as we have seen, it is occasionally necessary, in the rapid and fatal forms of disease to which, as a general rule, this treatment is adapted, to call in every auxiliary which may afford any relief, however temporary; still, any other drug, capable of producing any powerful impression on the system, is to be avoided as far as possible when the calomel treatment is being employed.[1] To a non-observance of this rule may be attributed a good share of the failure of those who have thought they were carrying out, in good faith, the precise rules recommended by the originators of the practice. Sir Henry Holland ("Medical Notes and Reflections") says, "Its good effects are much impaired by admixture with other aperients." It is still more impaired, as a general rule, by combination with opium, except when employed in the common method, that is, small doses frequently repeated, in which case the combination is rather beneficial than otherwise. The alleged success of Johnson, Annesley, Cunningham, and others, with the sedative doses, induced multitudes of other practitioners to try them, and to go much further than they ever deemed it necessary or safe to do, giving the scruple doses every hour, two hours, etc., as a routine practice. These writers take pains especially to denounce this abuse of the treatment, as they do the idea that ptyalism is the *sine qua non* of its success.[2] "But there is an abuse," says Holland, "also on the side of timid and deficient employment where its decided and speedy influence is required. This is often forfeited by using it in those small and scattered doses which harass the bowels," etc. It is not uncommon to hear physicians talk of having used the "calomel treatment," or to see written accounts of it, where the doses did not exceed ten grains, a dose too large for the good effect of the fractional-dose treatment, and too small to produce other than irritating instead of sedative effects. Another cause of alleged injurious effect of the large doses, and which I have not seen noticed, has probably arisen, in former days especially, from the uncertain quality of the medicine. Even at this day, when so much more attention is paid to the purity of drugs, we occasionally hear of calomel contaminated by various impurities, even by corrosive sublimate.

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[1] Such remedies as opium, laxatives, astringents, etc., are here referred to. Tonics, sudorifics, stimulants, nutritives, etc., are not only not contra-indicated, but often assist very much.

[2] See James Johnson, "Influence of Tropical Climates on European Constitutions;" and Annesley, "Sketches of Diseases of India."

The action of calomel, in large doses, has been constantly spoken of in this paper as a *sedative* one; and its initial, if not its principal influence over such forms of disease as have been here discussed, must be due to a tranquillizing action on the ganglionic system of nerves, as evidenced by the promptitude and character of its effect. But it is not claimed that this explains or comprehends, by any means, its entire *modus operandi*, in the ultimate cure. The explanation of this has puzzled much more able observers than the writer, and baffled much keener intellects than his. One fact, with regard to its action, we know from Annesley's experiments on living animals,[1] where one, two, and three drachm doses were given; and from Beaumont's observations in the case of St. Martin, that calomel relieves hyperæmia of the alimentary canal; and this explains, to a certain extent, its action in the affections of this membrane.[2] But its power over such a disease as croup is not so explicable. Dr. Rush thought that it acted by a counter-irritant effect on the intestines. But it is not, by any means, always necessary to carry it so far as to produce such counter-irritation, nor will that, produced by other and more drastic cathartics, produce any such result.[3] It has been long claimed that it diminishes the plasticity of the blood, and in so far may aid in subduing such a disease. Its action has also been attributed to sympathetic influence, such as is exerted between similar organs and similar tissues, as explained by Bichat. We know that an irritant to the conjunctiva of the eye will induce sneezing; that irritation of the *membrana tympani* will provoke violent cough; that irritation of the uterine mucous membrane will induce vomiting; and that vomiting, *per contra*, will induce contraction of the uterus, and thus check hæmorrhage; in fine, that it is not improbable that, along with its other influences, calomel acts on the mucous membrane of the *larynx*, through the intestinal lining, by means of that remarkable reflex phenomenon of the sympathetic system enunciated by Marshall Hall, and which, through the labors of Brown-Séquard and others, has rendered explicable many intricate physiological and pathological problems.

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[1] *Op. cit.*

[2] "Mr. Cunningham took, when in perfect health," says Johnson, "by way of experiment, three scruple-doses of calomel in one day." The sensations along the tract of the intestines he describes as rather pleasant than otherwise, and they were followed by "one natural stool in the evening."

It has been already stated that Billing claims for it a power of contracting the mucous capillaries, and that Headland denies it.

[3] Of course, a good deal of its beneficial influence in this and other diseases has been attributed to its *cholagogue* effect. It has, however, been recently demonstrated, by a long series of experiments carefully conducted, that, on *healthy* animals, it not only does not stimulate an increased flow of bile, but actually diminishes it. It is to be expected that so powerful an impression, as calomel is capable of producing, *might* interfere with the harmonious action of the abdominal viscera (in a state of health); and

In conclusion, and to guard against any misapprehension of the writer's views respecting the mercurial treatment of disease generally, and consequently of any misrepresentation of the same to his discredit, he begs to state that he is as much opposed to its indiscriminate use as an alterative, that is, in minute doses with a view to mercurialization, as almost any one can be. One of his first essays,[1] on entering upon practice, nearly twenty years ago, was designed to show on what a slight foundation the *prophylactic* use of mercury in primary syphilis stood. Yet, at that period, its employment for that purpose was very general, especially among the older practitioners.[2] He will go so far as to say that, with the powerful

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yet, as Dr. Ringer remarks (*op. cit.*) "may act in disease as a cholagogue by setting aside the conditions hindering the formation of bile." The liquefaction of the viscid secretions coating the *duodenum*, and their removal, which calomel effects, thus clearing the outlet of the *ductus communis*, a continuous action along this tube, and the consequent relief of a distended gall-bladder, may emulge the overloaded ducts of the liver, and thus *indirectly*, but perhaps no less efficiently, act as a cholagogue; whereas, in health, no such series of actions could be expected.

[1] Medical Notes and Observations, New York Journal of Medicine, 1852, p. 61.

[2] Headland, in his last edition, remarks that mercury is useful in both forms of syphilis; and Sidney Ringer, still later, remarks that it is still

modern remedies which enable us to promptly act on the nervous system, and to control the action of the heart, and which our predecessors a few years back had not, calomel, as an alterative, is, to say the least, unnecessary in all forms of fever, in most diseases of the liver and alimentary canal, and also in peritonitis, iritis, pericarditis, and allied affections, in the treatment of which this remedy has, until quite recently, been considered indispensable.[1] But, to ignore the powerful influence for good which calomel exerts in sedative doses, in some of the forms and phases of various diseases, and as a means of arresting the progress of certain diseases of a more than ordinarily dangerous character, especially where other agents are generally admitted to be futile for this purpose, and where we have such convincing evidence, from such responsible sources, scattered over our medical histories; to cast aside the *good* which so valuable an agent is capable of effecting, when properly employed, because the evil of its *abuse* has been so great; to yield to the clamor, which is rather becoming traditional, that to use calomel in efficient doses is an evidence of old fogyism, or of a failure to keep up with the rapid strides of medical science, is, in his opinion, scarcely less unphilosophical and reprehensible than to use it, as was formerly done a century ago. The best, the most potent of our weapons against disease, are, of course, those most liable to abuse. *Opium*, the greatest of all our remedies, *magnum donum Dei* of Sydenham, is an instance of this. The trained soldier, when he goes into battle, does not reject the most effective weapon, because it has proved unsafe in rash or unskilful hands. Neither should the intelligent and observing physician allow any prejudice or tradition, however honored by time or au-

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used by many physicians in both forms of the disease, and that it is supposed to render the secondary effects less severe.

[1] Nevertheless, the writer deems it just to say that the injurious effects of mercury on the system have been much exaggerated, as every one acquainted with the subject must admit. Niemeyer, in this connection (*op. cit.*), remarks: "An exceedingly rich experience has taught us that the innumerable multitudes of persons, who have used calomel more or less for other purposes than anti-syphilitic, have not suffered any permanent injury to their digestive organs, or their general health, but are now as healthy as they ever were."

thority, to stand between him and the judicious trial of any means of combating disease rebellious to our ordinary weapons, or our ordinary method of employing them.

Immediately after the adjournment of the Society, the writer was informed, by a member, that another member, Dr. Knapp, while practising in the eastern part of the county some years ago, had a case of croup under treatment, which was, to all appearance, in a desperate condition ; at this juncture, the friends insisted on calling in an old practitioner from Connecticut, who happened to be in the neighborhood. He immediately prescribed *teaspoonful doses* of calomel, and the child recovered after taking several. It is probable that this was considered a sort of accidental hit, by all the physicians acquainted with the circumstance, as the relator did not seem to have been at all impressed with the importance of the occurrence, until his attention was recalled to it by the facts related in the above paper. The manner in which the treatment of " croup " by " heroic " doses was introduced into the city of New York by Dr. Bay, or reintroduced, was almost precisely similar, and occurred in a case under the treatment of Drs. W. Parker and F. W. Johnston. It seems to have been considered rather as a "hit," than the result of previous experience. Another member mentioned the case of his own wife, in corroboration of the facts adduced in the paper. She was attacked, during the prevalence of the *cholera* in 1849, by dysentery ; wishing to check the discharges promptly, the doctor prescribed moderate doses of morphia. The discharges were not arrested before a peculiar affection of the brain set in, which the doctor ascribed to congestion (there were no evidences of narcotism). He immediately prescribed a scruple of *calomel*, not for any specific effect, but simply to control this symptom ; she was promptly relieved ; and, to his surprise, had no passage from the bowels until the morning after, and then a regular, healthy evacuation.

ART. II.—*On the Use of Force, applied as Vibratory Motion, in the Treatment of Diseases of the Nerves.* By GEORGE H. TAYLOR, M. D., New York.

I HAVE successfully employed *vibratory motion* in the treatment of paralysis and other forms of diseases of the nerves. both alone and with other remedies. The therapeutic value of this agency has therefore been thoroughly tested. I propose now to detail some of the results of the cases so treated, in connection with what appears to be the *rationale* of its action.

It will be seen that the effects of the agency are positive and unequivocal; they are easily secured and under complete control. The applications are very agreeable to the sensations of nervous invalids, the action of the agency accords with the facts and principles of modern science, and its future prominence in therapeutics appears clearly indicated.

The employment of direct force for therapeutic purposes may be regarded as novel; but a little reflection will show that, besides the chemical force involved in the use of drugs, force has been indirectly employed to a large extent in the forms of electricity and galvanism now regarded as indispensable therapeutic agents. Heat and the variations of temperature have also, through all time, been brought to bear on the organism for its therapeutic effects; and my present purpose, distinctly stated, is to show that force, in the form of vibrating motion, is not only remedial, but that it has direct control over some of the most unmanageable forms of chronic disease.

The reader's consideration is requested to a few facts of elementary science, in order to aid his understanding of the modes of action of this agency. He is reminded, meanwhile, that the field of rational inquiry is open, and that further experience may bring new facts to light. All investigation can but render more clear and decided the practical value of the principles to which attention is invited.

If a moving body come in contact with one at rest, its motion is arrested, but the force by which it is projected is not thereby obliterated, or even diminished. It has been demonstrated, by a great variety of experiment, that no portion of such force is lost, but that it is only caused to assume new forms. At the moment of the impact, the moving force is transformed to its equivalent of *heat*, *electrical disturbance*, in some cases, *light*, and perhaps other forms of force. The arts supply abundant illustrations of this principle, and scrutiny of the various conditions under which it appears demonstrates its uniformity.

If the body against which another is projected be composed of particles destitute of cohesion, it is apparent that the result would be modified, and the force would be distributed among the separate particles. The friction of the moving atoms of liquids, it was shown by Rumford, produced a large amount of heat, and both Meyer and Joule demonstrated the uniformity of the transformation of motion to heat, and their units of equivalence.

If, again, a fluid body be of complex composition, containing chemical elements of diverse nature, the conditions supposed are favorable to the manifestation of chemical action. The energy of motion in this case, in addition to the other effects, determines new arrangements of elemental matter, and new compounds are evolved in such a body. The test-tube of the chemist affords an infinite variety of illustrations of this principle.

The human body evidently embodies all the conditions above presented, and is consequently amenable to the effects described, on the application of similar causes.

We can trace rationally, as well as experimentally, the several effects, in the living body, of an impact or impression, such as is now considered.

1. An act like that described disturbs the bodily fluids in the following manner: Those contained in the vessels reached by the motion will, at the instant, be accelerated in their course, since the calibre of such vessels is diminished. The vital contraction of the walls of the vessels, stimulated by the impression, coöperates to increase and continue this effect. The valves of the circulating vessels will necessarily determine the direction of the flow thus produced.

2. Whatever physical obstructions may exist within the

capillary vessels are removed beyond the obstructed point, by the impulse thus afforded. Hence the aid supplied by such a cause, in the removal of chronic congestion.

3. The motion of the fluids within the vessels extends to and is the cause of motion of those exterior to them. An interchange is thus effected between these fluids, by the physical act of *endosmosis*, absolutely essential to the maintenance of nutrition. Certain indispensable chemical changes are well proved to accompany this act, and to depend upon it.

4. A large amount of heat appears, appreciable by the organism, though eluding observation, if produced by the same cause outside the body. This differs from all other applications of heat, in the fact of being produced *interiorly*. Heat otherwise supplied reaches central organs chiefly by way of the circulation, and with comparative difficulty.

5. The tissues and fluids of the body have, normally, a strong attraction for oxygen, that must be satisfied in order to maintain health. Ordinary exercise, or the use of power, is the means by which this is usually secured. But in chronic diseases the use of the powers of the voluntary system is diminished, and oxidation is abated in equal degree. Doubtless, also, by reason of alteration in the compaction and gravity of the bodily fluids, their affinity for oxygen is diminished in diseases. What is clearly wanted, then, is an increase of energy of these normal affinities.

Motion among these constituent elements affords not only direct but energetic contact of matters destined to chemical union. The feeblest action of this kind implies motion—and energetic molecular motion demonstrably intensifies and perfects chemical activity. Increased oxidation, hence, becomes inevitable in the system when motion is freely supplied to its contents. Practically this result is proved by the increased products of oxidation which pass from the body under such circumstances.

6. An important practical consequence follows the fact last stated. Increased oxidation diminishes the oxidizable materials in the system. It will be seen from the preceding statements that, in the chronic invalid, there necessarily are many intermediate products of waste, illy and incompletely

oxidized materials, and other adventitious contents of the body, which doubtless serve as the proximate cause of diseases. Practical medicine, impliedly at least, supposes such a condition. Now, increased energy of the normal oxidizing process converts such matters directly into the innocuous forms of carbonic acid, water, and urea, and, hence, purges the system of their presence, and relieves it of their effects.

But the human system involves other conditions in addition to those above described. These elemental changes and transpositions of matter and force have in them a purpose. The body is also a *vital* object, and it is the purpose of all its arrangements to sustain its vitality, and to evolve through its organized substance several forms of its own. Matters entering the body have, hence, a predestined career directed to this end. The preponderance of vital influence subjugates chemical and physical action to vital purposes. The vital molecule influences and appropriates the contiguous matters brought within the sphere of motion, and the successively changing atoms become in some way, at present unexplained, the bearers of the vital power that serve the purposes of the will. It hence follows, that the amount and quality of the power manifested through the organism are entirely dependent on the action of these chemical factors. The most useful remedies must be those that act in the direction of organic tendencies at every point of the course of matter in the system. Hence, the agency of motion is the direct aid to the fulfilment of the vital tendencies of the system.

It will be observed, that the supply of motion to the constituent elements of the body (if in suitable degree) *introduces no new form of action.* It only exalts the value of that previously existing, by rendering it more pronounced and effective. In other words, force supplied from without is, by physical, chemical, and physiological transformations, made to subserve vital uses, and render them more perfect. "We everywhere discover," says Claude Bernard, "a rigorous correlation between the intensity of physical and chemical phenomena and the activity and phenomena of life; therefore, we are able, by acting on the first, to modify the second, and regulate them at will."

From the above, it will be seen that the consequences of a single impact of forces upon the organism are distinctly traceable in certain inevitable physical, chemical, and physiological effects, in the inmost recesses of the body, wherever vital power is evolved. That such a cause influences and controls the evolution of this power is hence a logical necessity.

The energy expended among vital molecules by any one impression or impact, as above described, is, however, too inconsiderable to be appreciable in a therapeutic point of view. *Vibration*[1] is but the repetition of such an act in an unlimited series. The effects are then, of course, correspondingly increased, and conspicuous, in at least the various particulars enumerated. The therapeutic bearing of these effects will depend on various circumstances quite under the control of the physician. These are, briefly, the form in which the vibratory action reaches the tissues, the rapidity of the impressions, the regions selected for their action, and its relation to other parts, the degree of force with which impingement is made, the direction in which the vibratory wave is sent, and the order of succession of the different parts subjected to the various forms of the operation. All these particulars are subject to an almost infinite variety of modifications and requirements of the system.

The vibratory motion is applied to a limited portion of the body at any one time. Mechanical instruments, contrived with reference to various effects, are employed as the medium of force, and are operated by some constant and adequate power. These instruments are so arranged that the applications may be instantly and properly directed and qualified. The adjustments of the various conditions mentioned are quite under the control of the one using the application, and nothing in any way uncomfortable is tolerated.

Having seen that certain effects of decided therapeutic value are produced by molecular motion, the causes for which are supplied from without, it remains to inquire what special advantages inure to the cerebro-spinal axis from this cause, in case of disease.

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[1] See article in NEW YORK MEDICAL JOURNAL for November, 1868, entitled "Inquiries relating to the Therapeutic Effects of Vibratory Motion."

1. The most prominent among these are included under the term *revulsion.* There are two varieties of this effect. One consists in securing an afflux of blood to some special region of the body, thus diminishing it in the other portions; while the other relates to the kind or order of functional activity the nutritive fluids are made to sustain.

The mode of securing revulsion of the first variety is readily understood. As before stated, the applications are made to a limited portion of the body, and the increased activity of any region from this, more than from any other cause, increases the measure of blood which flows thitherward, and becomes fixed therein. This effect is attended by a corresponding diminution of blood in contiguous, and even in remote regions of the body. This revulsion, consequence of motion, may be increased to any desired extent by simply subjecting contiguous portions successively to the operations in their different forms.

While new and vigorous action is assured in any chosen part, such part is thereby caused to assume new relations to the remainder of the body, as regards the distribution of nervous power. While such power is stimulated in the region of the application, like the flow of the blood in the same region, it is diminished at points of various degrees of remoteness from the excited region. In fact, the distribution of nerve-action appears to follow closely the nutritious actions that are so easily and thoroughly excited by vibratory motion.

The practical value of the revulsive effects now described are demonstrable equally in the more important affections relating to extended regions, as in the control of the lesser ailments of invalids. Thus, various disabilities of the head, chest, digestive organs, pelvis, etc., find relief in this recourse; but nowhere is the effect more complete and more satisfactory than in case of diseases referable to the cerebro-spinal axis. These parts, if suffering from congestion, experience direct relief, as a consequence of the excitement produced in the skin and extremities by vibratory motion. This effect is rendered permanent by the repetitions of the operations at suitable intervals, as there are no coincident pathological conditions superinduced.

But this method of securing revulsive effects distinguishes itself above all others by the fact that the effect relates to functions as well as regions; and excited action of cerebro-spinal centres may by this means be subdued. Indeed, it has sometimes happened that persons of habitually excited nerves lapse into a quiet sleep while subjected to vibratory operations.

The reason for the above stated effects would appear to be this: normally, *muscular* action is preceded and accompanied by *nerve* action, either through the will or reflex causes. Now, in cases of vibration as above described, the action, however energetic, takes place in the absence of either of the above-named causes; consequently, the nerve-centres, which otherwise act coincidentally with muscles, are entirely relieved of that necessity. Hence, the nutritive effects which action always calls for are determined upon other tissues. The nerves are therefore relieved of preëxisting tendency to congestion, superinduced by their too great habitual activity. In this way, the most prominent probable cause of nervous disease is removed. For it is a matter of popular observation that persons, whose habits of life subject the nerves to undue activity and wear for a long period, incur a far greater liability to nervous diseases than others; indeed, they are always verging on some form of such disease. Constitutional predisposition has also its place among such influences. This unequal use requires and compels unequal nutrition.

Although the brain of man is but one-thirtieth of the weight of the body, physiologists have shown that it receives from one-tenth to one-eighth of all the blood sent from the heart to sustain the body. If the afflux be too much increased in consequence of little or no counterpoise in other kinds of functional activity, the nerve-centres will be under perpetual pressure. If now the general health become from any cause impaired, through the respiration, digestion, or any of the ordinary channels of derangement, it is plain that the deterioration must be more decided and grave at the point of greatest nutritive action, and disease of the nerve-centres is the inevitable consequence.

For such a condition, the remedial indications are perfectly obvious. The nutritive afflux to nerve-centres must be turned in the opposite direction. To increase the healthy action of muscles and other physiological elements is to relieve embarrassed nerves. This therapeutic indication there is no other proper way to meet.

But the indication is *not* met by ordinary exercise, because this implies *effort.* Such exercise commences in volition, which has its origin in the cerebral centres. In health, the coincident use of nerve and muscle preserves their equipoise; but in disease of the nerves there is inaptitude for the action of the volitions to any considerable degree; and such exercise is damaging, because, instead of restoring the equipoise, it increases the disparity of action between these two great physiological constituents of the body—the muscles and the nerves.

There is reason to believe that many of the usual remedies employed in nervous affections serve really to increase the inequality of nutritive action, and therefore to perpetuate, while seeming to relieve, the disease.

The principle of revulsion has from time immemorial been an indispensable therapeutic recourse. Even when therapeutic action has been regarded as of some other kind, it is probable that it has often been of a revulsive nature, and that many apparently conflicting remedial methods can be reconciled in this view of remedial action. It is my own belief, however, that no other means has secured the effect in any thing like equal degree or permanence, as that resulting from properly applied vibratory action; while this has the advantage, over all other methods, of producing no morbid effects at the part of application, and of always securing the effect in the ratio of persistence of the application.

2. Physiologists agree that it is through chemical changes in the instruments of power that vital force is evolved. This is equally true, as we have just seen, of the *nerves*, the instruments of the will and sensation, as of the *muscles.* But the power thus liberated through "metamorphosis of tissue," it is calculated, is in excess of that expended on objects outside the body. There is, hence, a residual of force, which is appro-

priated by the organizing instruments, and employed for *constructive* purposes.

This subject is fully elaborated in Dr. Carpenter's work, entitled "Correlation of Physical and Vital Forces."

The bearing of this principle of physiology will be seen when it is understood that the molecular and chemical change superinduced by vibratory motion, while it liberates force, does so without its usual expenditure. The exertion of the will, the usual cause of expenditure of nerve-power, is entirely suspended, thus at once diminishing to a minimum the amount of such expenditure, and allowing the *constructive* force, generated in the manner above described, to be employed wholly for the benefit of reparation of the instruments of nerve-power. Thus it appears that the demonstrated fact, that vibratory motion aids the power and sustains the health of the nerves, is also sustained by theoretic considerations, emanating from a leading authority.

3. It is quite probable that the certain and rapid development of *heat* among the vital tissues, secured by vibratory motion, may serve to facilitate the development and perfection of the vital forms that constitute the instruments of vital power. This suggestion is supported by the well-known importance of a supply of heat for the germination of seeds, and for the development of eggs of all oviparous animals. In both these cases the development of vital activity is entirely dependent on the supply of heat. Physiology has hitherto afforded us no distinction between the elementary vital forms that are to serve in the animal structure as the instruments of its power, and those that are destined to be further developed into distinct and separate organisms; and it is reasonable to suppose that similar conditions are equally favorable to development in either case.

In chronic disease, there is deficient heat production, and it is all-important to recover the necessary condition for the renewal of tissue. Vibratory motion supplies it in at least two ways: by increasing oxidation, and by the direct conversion of motion into heat, through the attrition of both organic and atomic elements. While the latter affords an immediate supply, at once grateful to the senses and fructifying to the organic endeavors, through which the full amount needed becomes ultimately supplied, the former action, by changing the chemical character of the constituents of the system according to organic requirements, removes all impediments to organic action, or cell-growth.

The subjoined cases are purposely selected of different manifestations of nervous diseases, so as to show that the remedy produces its effects by influencing primary causes; that it goes back of the *expression* to the *essence* of such disease.

It can scarcely be disputed that the amount and quality of nerve-power, however manifested, depend on the facility of the reproduction of the cells of the nerve-centres, since our present knowledge of this matter regards these as the source of such power. While, then, the more obvious pathological conditions, such as mal-distribution of nutrition, restricted heat-production, congestions, etc., are being corrected by the remedy, the more obscure impediments to the evolution of power by the nerves are at the same time removed, and the health of the power-generating cell is restored.

The following is an aggravated case of cerebral congestion, and is a good illustration of efficacy of this remedy when it is employed for its purely revulsive effects:

Case I.—Mrs. B., aged about 40, married; had suffered several years from distressing and apparently threatening symptoms of the head. She is a person of full habit of body, with short neck and large head, and has much color of the face, bordering on purple.

The sensations were those of pressure in the head, nearly constant, sometimes aggravated to an almost unbearable extent, producing temporary loss of power, and often confining her for several days to her room and bed. Her feet were habitually cold, and no ordinary means were sufficient to maintain their comfortable temperature.

She had for more than a year been under the advice of the best medical talent, but unfortunately medicine supplies no adequate, certainly no *permanent* remedy for this particular class of cases. She was consequently often in a despairing state, since the frequent transient loss of power and consciousness constantly threatened to assume a permanent form.

In this condition, she applied to me in the winter of 1868. The revulsive influence of vibratory motion was fully brought to bear—first at the extremities, and gradually subjecting all parts of the body, except the head and neck, step by step, to the same influence. The relief she found from these measures was satisfactory from the first. After four weeks, having had no recurrence of the old exacerbation, and being quite free of all unpleasant symptoms, the treatment was suspended. A few weeks later, on the recurrence of some slight indications of the old malady of the head, she again resorted to the treatment for a short period, with complete success, since which she has remained well.

In *sciatica* and other kindred symptoms of diseased nerves, the effects of the treatment are equally pronounced. In such cases, restoration is probably effected, partly by the mode of revulsion first mentioned, but more decidedly by the second. The case of sciatica, now referred to, is but one of a goodly number in which the same result has been secured.

Case II.—Mr. N. of New York, merchant, aged about forty-five, had suffered for more than a year with sciatica of the left leg, which he described as greatly aggravated by harassing business cares. The pain had been nearly constant, but fluctuating in degree, and had compelled him for most of that time to rise from his bed, often several times during each night, to get relief. He had been treated by the usual methods, among them rubbing and electricity, applied by well-known physicians; only temporary relief, however, was obtained. He resorted to me in the fall of 1869. The affected leg was shrunken, and the flesh very soft and flabby. The treatment was applied with reference to increasing the muscular nutrition of the affected limb, while it should diminish the amount of blood in the lower segment of the spinal cord; that is to say, the patient was required to *do* nothing, while energetic molecular activity was aroused in the extremities, in the unaffected as well as the affected limb. Experience has proved that, for the *first* described effect of revulsion, the calling of the circulation into both limbs is better than into one. At the same time the limbs were profoundly irritated by appropriate processes, without, however, engaging the will in the affected leg. The symptoms soon began to abate; and after two or three weeks of daily treatment, employing it on the whole body, as well as on the affected limbs, he began to sleep well, and the pain, when it existed at all, was trifling. He continued the treatment for two or three weeks longer, when the need for it disappeared altogether.

The following shows the curative effects of vibratory motion in a case of complete and prolonged *paralysis.*

Case III.—The case of Miss Y., aged about twenty-four. Her affection came on gradually and insidiously, commencing immediately after recovery from a severe typhoid fever, which occurred five years before I saw her. The loss of both sensation and motion of both lower extremities, in about a year from the commencement of the symptoms, became almost complete. She suffered no pain, except occasionally in the lower region of the back, attended by a sensation of heat. Her only mode of locomotion was the wheel-chair, often used by this class of invalids. There was an almost entire absence of sensation in the feet and legs to the knees; above these was a gradual increase of sensation to a point above the hips, where it became perfect; the control of the sphincters was incomplete; she could barely stand on two crutches. While looking at her feet, if her attention was withdrawn, she was liable to fall.

The vibratory treatment was applied freely to her feet and legs first, afterward to all other parts of the body. This was done for two or three hours in succession, including intervals of rest, every day. Increases of the temperature of the extremities appeared from the first, soon followed by symptoms of increase of sensation and of power. The tendency to unpleasant heat in the back was subdued from time to time, as occasion required, by the *ice-bag*, applied for an hour over the region where this sensation was felt. This treatment was kept up, with slow but constant improvement, for four months. At the end of this time she was able to walk with a cane about the house, and occasionally make short excursions out of doors, with an attendant. This was in the spring of 1868. She went to her home out of the city during the summer, returning again in October, having made considerable progress in the interval. She now received the treatment again for a similar length of time, when her power was so far recovered as to afford no indication of any deficiency by her style of locomotion, by her sensations, or any other ordinary test.

This case had been treated most of the time for *five years*, by nearly all the ordinary approved methods of general as well as special therapeutics, without any success whatever, and was considered incurable.

The favorable result above narrated was, in my opinion, due to affording more energy to the nutritive processes of the lower segment of the spinal cord, and to the constant revulsive effect secured to it, both of which indications were liberally met by vibratory motion, judiciously supplied.

It is important to remark, for the encouragement of invalids of this class, that the result obtained in this case was owing to due perseverance on the part of the patient. If she had become discouraged at the end of two or three months, as a less hopeful person might have been, only negative results would have been attained.

Case IV.—The following case illustrates well the effect of vibratory motion in severe general paralysis of a person long predisposed to the affection.

Mrs. E., a widow, aged sixty-two, became a subject for my method of cure in September, 1867, four months after stroke of paralysis, affecting in nearly equal degree the whole system. At this time she was unable to either turn in bed, rise from a seat, articulate distinctly, use her hands to feed herself, or her feet to walk. Every change of position required assistance, and at times there was great difficulty in swallowing. Her symptoms had scarcely ameliorated since the occasion of her disease. One half of her body was defective in size and power, as the result of infantile paralysis, and had always been the seat of much neuralgic pain. She was subject to frequently recurring fits of involuntary sobbing, and at all times suffered from great depression of spirits; and was quite as hopeless of recovery as were her friends.

In this case the antecedents, as well as symptoms, seemed to point to a cerebral cause. The caprices of fortune had borne severely upon her, and she had devoted herself to literary labor with more ardor than could be well borne, which was the probable cause of the precipitation of her attack.

After three or four weeks of careful application of the vibratory treatment, sending the several forms of this action through the extremities first, directly followed by the same, through the central portions of the body, she began to show the desired evidences of improvement. The treatment was continued more than three months, when she had acquired strength sufficient to enable her to walk, and even to mount stairs. Her voice returned, and she could employ her hands in light occupations. The regular applications of treatment were now discontinued, but she at irregular intervals resorted to our rooms coming from her home, and so continued the improvement so well begun. After a few months she was so far improved as to engage in house-keeping and other usual female avocations, and to resume the vigorous exercise of her pen. She continued quite well at last accounts.

A rational inference, in regard to the cause of restoration in this case, is the removal of effused fluids. Vibratory motion, through the operation of causes before specified, produces *absorption*, on the return of interstitial fluids to the venous circulation. The effect of this, in such a case, is to remove the surplus of fluids in the sub-arachnoid spaces, and the consequent pressure from the brain and spinal cord. The well-proved oxidizing effect of vibration, and the aid afforded through the same cause in sustaining and reproducing the organic instruments of vital power, are to be kept in view, in accounting for restoration in these cases.

CASE V. *Hemiplegia.*—This is the case of Mr. S., a gentleman well known in public life, aged fifty-nine. He is of an extremely active, sanguine, irrepressible temperament; had been the subject of great extremes of excitement in the arena of politics as well as business, a thoroughly "used-up" man, whose general appearance indicated twenty more years than he had seen; in short, one of that class of cases for whom the physician expects least benefit from treatment.

The occasion of his disease was sudden, affecting his right side. He could walk a few rods with difficulty by giving the affected leg a lateral swing, having no control of the flexors of the foot. The right arm hung useless by his side; he was unable to raise it, or to move his hand or fingers, except, perhaps, in the slightest degree. The hand and fingers were numb, rigid, and swollen, and could not be straightened by the help of the other hand. There was, as is frequently found in these cases by careful examination, a dull tenderness of the *left* side of the base of the back of the head. The mental indications of disease were usually prominent in this case. He had often an irrepressible disposition to weep, great fear of being left alone, and general lowness of spirits, that frequently gave him intense misery.

He came to me about ten months after his attack—in the spring of 1867. All other treatment was suspended, and he was subjected to the various forms of vibration. The arm and hand being perfectly useless, special pains were taken to reawaken power in them. An unusually small amount of treatment satisfied him (contrary to the usual inclination of these cases as regards this form of treatment), but it was daily and regularly applied, according to our daily judgment of his condition. In three weeks from commencing treatment, he could for the first time hold his knife and cut his food, and also began to employ his right hand to open doors, etc. He returned to his home in six weeks, much improved, both in body and mind. He came back for a few days or weeks at a time, for further treatment, as suited his inclination or convenience. He became able to walk long distances in the streets, though not without some awkwardness in the control of the right foot, acquired perfect control of *all* the muscles of the right arm, though not its pristine vigor, and his use of the pen was nearly as perfect as ever. Some months ago he went to Europe with his family.

The results in this case certainly exceeded all reasonable expectation, when its nature and the temperament of the individual are duly considered.

Case VI.—This was a singular case of locomotor ataxy, in T. N., aged about fifty. He had pursued an energetic and successful business career for a long period, involving the cares and responsibilities of both manufacturing and mercantile business. The symptoms of disease developed progressively for two or three years. He resorted to me in the winter of 1866. I found the numbness of the feet and legs quite complete. A pin passed its whole length into the flesh elicited no sensation whatever. He did not feel the touch of his feet on the ground, and, having full muscular power, was obliged to maintain his balance in walking by placing his feet wide apart, and continually looking at them.

At the same time, he suffered much torture from morbid sensations. The chafing of his pants was annoying, while the casual brush of ladies' skirts against his legs, in passing, often gave great suffering. He also had irregular attacks of stinging, burning pains, which he compared to the feeling that might be caused by the impinging of a jet of steam; generally confined to a limited surface of the limbs. This form of pain frequently shifted its location. Common sensibility remained quite absent during these frequent paroxysms of excited and morbid sensation.

He had in this way been quite disqualified for business, receiving meanwhile the advice of the most esteemed in the profession. His last adviser, before consulting me, was Dr. Brown-Séquard, but nothing had served to arrest the steady progress of his affection.

No seeming benefit appeared to result from the application of the vibratory treatment for several weeks, unless, indeed, his ceasing to grow worse be so regarded. Considerable difficulty was found in properly adjusting his treatment to the seemingly mixed and contradictory indications. Should the action of the spinal cord be increased or diminished, and in what portions of it are these different effects required? It may here be remarked, that it is always *safe* to employ the treatment revulsively at first, and the period when the patient can receive with advantage increased action in the nerve-centres will infallibly be indicated.

Having thoroughly, and, in my own opinion, incontestably demonstrated the power of the remedy, in controlling morbid sensibility, the treatment of the case was persevered in. The tactile sense began at length to appear, and the paroxysms of pain to diminish in number and severity. At the end of four months, when he discontinued the treatment, the sensations were nearly normal, a slight numbness remaining only in his feet. He revisited me a few months subsequently, to show that his health was in every way quite restored, fully qualified for business, which he has resumed, relieving himself only of excessive attention to details. He still continues well.

In this case there were both anæsthesia and hyperæsthesia, in extreme degree, giving rise to grave suspicions of the organic integrity of the spinal cord. The result proved that vibratory motion, applied tentatively to all parts of the body, is adequate to correct the morbid irritation of the spinal cord, and thereby to restore the integrity of its functions.

In *neuralgia* and *hyperæsthesia* of various organs, the vibratory treatment is attended by similar favorable results. The following case of prolonged hyperæsthesia of one leg is a good illustration of the effects of this agency in the class referred to.

Case VII.—The right leg in this case suffered from morbid sensibility, always present, and so acute at times as to render the touch of another person exceedingly painful, and the consequent pain would often be prolonged for many hours. The sensibility was not confined to any particular region, but was common throughout the leg, though greatest in the foot. The whole limb was much shrunken, soft, and flabby. She always required the aid of crutches for locomotion, and was unable to mount stairs, even with their use. Much of the time prolonged pain in the foot would follow if it were allowed to rest long upon the floor. A notable characteristic in this case was that the pain of her limb was always aggravated by any mental action. An increase of pain would be caused by five minutes' close attention to a subject. Even social conversation of any interest was sufficient cause to aggravate the painful condition, soon compelling her to desist. This condition had continued with unimportant variations for about twenty years. She is a lady of strong and active intellect, which she had always been in the habit of severely taxing. The affection commenced at about the age of thirty, when she had for some time been engaged in the severe duties of teacher, in connection with the pursuit of exhausting literary occupation.

The affection was not thus prolonged through the lack of medical attention. She not only obeyed the advice of different physicians of well-known eminence, but had freely availed herself of all sorts of special remedial treatment through all these years, not excepting "movements."

All of these applications seemed, however, rather to aggravate than to benefit the intolerable sensitiveness of the affected limb. Having demonstrated in several instances the control secured over similar symptoms by my methods as here described, I was desirous of trying their effect in so grave and obstinate a case as this, notwithstanding its discouraging history.

The treatment was so applied as to secure its *revulsive* effect, both as respects regions and functions. At first, and for some time, the painful limb was left untouched, while the various forms of vibration were sent through the unaffected limb, as well as through the body, in all directions. This was aided by treatment applied by the hands of assistants to the spine and other portions of the body. In a few weeks we had the gratification of finding a diminution of the painfulness of the affected limb, so that in five or six weeks she began to tolerate the treatment when applied directly to the suffering extremity. She continued the treatment quite irregularly, however, through the winter, at the end of which time her crutches ceased to be of service except on occasions of fatigue, when some degree of the old suffering would be renewed. After an interval of several months the treatment was resumed; the weak limb acquired hardness and strength till it became equal in every respect to the other. She has not used her crutches for the last four or five years. This lady is now, at sixty-nine years of age, more energetic and healthy than she has been since her early womanhood.

In discussing the value of vibratory motion in nervous diseases, I have avoided, as far as possible, statements involving a theory of pathological changes of nerve-centres corresponding to different forms of disease, and have been content to base the *rationale* of cure upon well-settled fundamental principles, no longer subjects of controversy. Indeed, all pathology in its last analysis is resolved into *perversion of nutrition*, and it is at the correction of this elemental action, at the point of generation of vital power, that our remedial agency is really aimed. Its radical effects depend on this radical purpose. Those ocular and tangible pathological results that are revealed by the *post mortem* and the *microscope* are but the last results of insufficiency and feebleness at this primary point of action, and can be really corrected only by supplying more favorable conditions at the point of departure from the healthy type of vital manifestation.

ART. III.—*Physiological Effects of Lightning.* Resumé of a Treatise by Prof. ANDRÉ POËY. Translated and condensed by JAS. D. BELL, New York.

IN his recent eloquent work, Mr. Lecky remarks: "The medical powers of electricity, which, of all known agencies, bears most resemblance to life, are almost unexplored."[1] It must be acknowledged, with pain, there is but too much truth in this assertion. By reference, however, to this JOURNAL for November,[2] it will be seen that something is being done to remedy the defect. The present contribution (extracted from an extended work written by the author in 1855, but still unpublished), has the same object in view. Its brevity must be attributed to the writer's engagements precluding his working up the materials at hand, rather than to any dearth of such materials.

I.—*Reduction to Dust of Thunderstruck Bodies.*

Dr. Boudin, in his first memoir upon lightning, remarks: "People say, 'If one touches the body of a person killed by a thunder-bolt, *it falls to dust.*'" "'To dust,'" adds this *savant*, "is an error; but does not the word *fall* seem to indicate that people have an idea of death while standing *erect?*"[3] How-

[1] *History of European Morals.* New York: D. Appleton & Co., 1869, vol. i., p. 166.

[2] See p. 210. Mr. B. W. Richardson's Experiments on the Effects of the Lightning Stroke on the Human Body. A continuation of these experiments is recorded in the present number of the JOURNAL.—ED.

[3] Boudin. *Annales d'hygiène publique*, etc., 1854, 2e série, t. ii.,

ever, more exhaustive research having convinced him that *incineration* of thunderstruck bodies, though rare, is none the less real, M. Boudin, in his second memoir, rectifies his opinion, by relating the titles alone of two cases of this kind.[4] Unhappily, M. Boudin has committed a double error: first, the two indications given by him from the *Bibliotheca Britannica* refer to the same case published in 1613 (not 1637), by John Hilliard;[5] his second error is, the individual in question was not suddenly reduced to ashes by the thunder-bolt, but was slowly consumed after being struck by it. It is even probable that he suffered spontaneous combustion analogous to what persons excessively addicted to *spirituous liquors* occasionally undergo.

Here, however, are three more exact cases of incineration: "A thunder-bolt has been seen," says Barberet, "to split a tree without even blackening it, while it *reduced to ashes* a shepherd who had taken refuge under its branches."[6] . . . . . "One man alone was killed," says Abbé Richard, "and *almost reduced to dust*, in the midst of many others who were under the same tree."[7] . . . . "The most curious as well as the most recent instance is the following: At Vic-sur-Aisne, in 1838, a thunder-bolt struck a lime-tree, under which three soldiers had taken shelter; they were all killed at the same instant; still, although lifeless, the three *remained erect*, as if the electric fluid had not reached them, their clothing also appearing uninjured. But when they were touched, all fell, so it is said, into a *heap of ashes*."[8]

I will now present the opinion of four writers upon this subject. Colonne says: "The fire of exhalation which forms the

and p. 27, du tirage à part. *Traité de géographie, etc., médicale.* Paris, 1857, t. i., p. 524.

[4] Boudin. *Annales*, t. iii., and p. 43, du tirage à part.

[5] Hilliard (John). *Fire from Heaven burning up the Body of one John Hitchell*, etc. London, 1613, 4to, of 17 pages.

[6] Barberet. *Dissertation sur le rapport qui se trouve entre les phénomènes du tonnerre ceux de l'électricité;* couronne par l'Académie des Sciences de Bordeaux en 1750, 12mo, p. 11.

[7] Richard (l'Abbé), *Histoire des météores.* Paris, 1771, t. viii., p. 250.

[8] Pylaie (de la). *Effets extraordinaires de la fondu au bourg de Beitz*, le 25 Octobre, 1838, Senlis, p. 6.

thunder-bolt must be subtile and so penetrating, that it kills a man by drying up instantaneously all his humors, without in the least changing his figure. We do not perceive him dead until we *touch* him, and then *we see him fall to dust.*"[9] . . . Gronberg remarks: "It is demonstrated that lightning reduces bones *to ashes*, but never to the state of fusion."[10] . . . . Izarn observes: "It is proved that lightning reduces bones to ashes."[11] His addition, that it never fuses them, is an error, as I have a case of that kind, as well as one of the softening of the bone. In fine, Abbé Mann asserts: "Lightning reduces to ashes all parts of the human body, without altering the clothes or skin."[12]

Besides the complete incineration of persons, or of portions of bone, and of trees struck by lightning,[13] we have also found that of some inorganic bodies, such as the following: a lady's *bonnet;*[14] a man's *shoe*, except the wooden sole;[15] *panes of glass* in windows;[16] a *cloth* covering shields which were fused;[17] a *purse*, the gold and silver which it contained not being damaged;[18] the *scabbard* of a sword;[19] a large *beam* reduced to dust in the middle of a chamber closed and fastened on all sides, without any injury to the chamber;[20] the different *materials* of a house destroyed by lightning;[21] the wood of a *cask*,[22] etc.

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[9] Colonne. *Histoire naturelle de l'univers.* Paris, 1734, t. i., p. 109.

[10] Gronberg. *Journal de Physique* de l'Abbé Rozier, 1773, t. ii., p. 559.

[11] Izarn. *Lithologie atmosphérique.* Paris, 1803, p. 78.

[12] Mann (l'Abbé). *Anciens mémoires de l'Académie des Sciences de Bruxelles*, 1783, t. ii., p. 289.

[13] Desormery. *Comptes rendus de l'Académie des Sciences de Paris*, 1849, t. xxviii., p. 137. Hartsoeker. *Conjectures physiques.* Amsterdam, 1716, p. 346.

[14] Arago. *Œuvres*, t. i., p. 287.

[15] Boudin. *Annales d'hygiène*, etc., 2e série, t. iv., and p. 55 du tirage à part.

[16] Arago. *Œuvres*, t. i., p. 284.

[17] Musschenbrooke. *Cours de physique expérimentale.* Paris, 1769. t. iii., p. 419. Hartsoeker, work cited, p. 346.

[18] Mann (l'Abbé). Mémoires cited, p. 289.

[19] Bertholon (l'Abbé). *De l'électricité des météores*, t. i., p. 113.

[20] Hartsoeker. Work cited, p. 346.

[21] Gauthier de la Lapeyronie. *Voyages en Islande*, trad. du Danois: Paris, 1802.

[22] See note following (23).

The mechanical fusion effected by lightning, or the *incineration* of thunderstruck bodies, were also known to the ancients. In the distinction of the different kinds of lightning made by Pliny, he mentions separately those which are *dry*, which do *not burn*, but which disunite and *reduce to dust.* The exactness of Pliny's observation is very striking, for, if we now substitute for the property of dry lightning, that of the *mechanical effects* of lightning without the production of heat, we have the same phenomenon under a new name. Seneca says also that lightning reduced to *ashes* the wood of a cask, which remained in place, and thus kept for many days the wine from being spilled.[23] Is there not here an identity of cause and effect, with the case of the incineration (mentioned above) of the persons killed by lightning, who retained their position until touched? Do we not also see, in a forest, the branches and trunks of trees, apparently preserving all their vitality without alteration of form, reduced to dust in the hand when grasped? Well, lightning produces analogous effects, as we have just seen.

Admitting for the moment that these effects, apparently surprising, are authentic, we will examine the explanation which the new data of science furnishes us. We see at the outset that the incineration by lightning of organic and inorganic bodies appears not to differ from the pulverization of metals by discharges of batteries of a certain intensity, such pulverization not being due to a true fusion, as M. Riess has demonstrated, but to a strong *molecular disgregation.* It is, therefore, a *cold fusion*, as has been conjectured by Franklin, or a *mechanical effect* of such intensity, that the molecules have not time to be raised to the degree of fusion, nor even to acquire heat enough to ignite combustible substances. It is by reason of this that fifty hectolitres of wheat having been lifted at a single stroke by a discharge of lightning, and dashed against the ceiling, fell in a shower of dust upon the floor of the apartment *without being ignited.*[24] On another occasion lightning carried, without ignition, from end to end of a granary, the *oats* which it contained.[25] In a word, the reduction to ashes or to dust of thunderstruck bodies must be explained by a sudden and violent disgregation of the molecules of the body itself, without the manifestation of warmth caused by overheating, any combustion whatever, nor even a carbonization. The body, having suffered a complete molecular disgregation, can yet preserve both its form and its attitude as long as it is not touched, because the power of the discharge, as an electrical force, is wasted in the work of disgregation, without leaving sufficient energy to separate the parts of the body which are now held together only by the juxtaposition of the molecules.

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[23] Seneca. *Questiones naturales*, chapter 31. L'Abbé Richard, work cited, t. viii., p. 43.

[24] Boudin. *Annales d'hygiène*, etc., 2e série, 1855, t. iii., and p. 27 du tirage à part.

But, as the modern principle of the conservation and transformation of *vis viva* (according to MM. Mayer, Joule, Grove, Helmholtz, Seguin, and other physicists) opposes the complete disappearance of one of them, it must be added that the electrical force, after having produced the mechanical disgregation of the molecules, is transformed into heat by the effect of the shock, which heat has served to evaporate the liquids and gases of the human body, and that, in fine, this evaporation has been also followed by a sudden refrigeration, which has contributed in its turn to keep united by juxtaposition the molecules which were before held together by chemical affinity subordinate to the vital force. The immediate disappearance of thunderstruck persons,[26] without leaving a trace of their bodies or any of its parts, is an effect of the same kind, due to the greater dynamical force of the bolt. Thus the electrical force of lightning, by producing a molecular disgregation, is transformed into heat, which, having vaporized the liquids and gases of the body, gives place to a sudden refrigeration. This rapid transformation of electrical energy into forces, mechanical, calorific, of evaporation, and of refrigeration, has preserved the equilibrium of the molecules, and the body, reduced to *ashes*, consequently preserves its attitude.

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[25] *Histoire de l'Académie des Sciences de Paris*, 1747, p. 319.

[26] Meureri (Wolfgangi). *Medici et philosophi meteorologia questionibus informata.* Leipzig: 1587, 4to, p. 140. This work, entirely forgotten, contains much interesting matter.

II.—*Congelation of Persons killed by Lightning.*

General Comte de Maistre, son of the illustrious author of *Soirées de Saint-Petersbourg*, informs Dr. Boudin that some Sardinian sailors killed by lightning in the Mediterranean presented all the signs of death by *freezing*. "This observation is the more remarkable," adds M. Boudin, "as it recalls the congelation of wine long ago noticed by the ancients."[27] On a man struck by lightning toward the back of the head, at the passage of the stroke, there could be found neither fracture, nor fissure, nor yet an alteration in any other part of the skull. The brain was also very sound, there being alone at the superior part *congealed lymph*, which had infiltrated into the folds of the *pia mater*. This young man, killed by the stroke, remained *immobile*.[28]

If the congelation of persons killed by lightning has been well observed, if it has not slipped in by some illusion in the observer's mind, and if especially this fact comes to be confirmed, here is the explanation of it which I would propose: The recent researches of MM. Joule, Clausius, Thomson, and others, have led physicists to regard heat as resulting from a rotatory motion of the particles about an axis, and M. de la Rive has considered this hypothesis as calculated to explain the polarity of atoms. This *savant* then asks, "if it might not be possible that the cause of the heat generated by the reunion of the two electricities, brought about under the form of discharge or current, may be not in the fact itself of this reunion, but in an increase of molecular motion, which might result from the transmission of electricity?"[29] Consequently, according to this theory, a current or an electrical discharge directed upon a body, must give rise to two molecular motions in contrary directions, and in the sequel to effects equally inverse. In the first case, the current being directed *in the same way* as the polarity which produces the natural heat of the body, it is evident that it augments the rotary motion of the particles around their axes, raises their temperature, and produces in proportion to its energy new thermo-mechanical effects. The result, therefore, will be the elevation of the temperature of the body traversed by the current or electrical discharge. In the second case, the current being, on the contrary, *in an inverse direction* from the polarity which produces the natural heat of the body, it is also evident that it diminishes the rotary motion of the particles around their axes, lowers the temperature, and produces, as in the first case, thermo-mechanical effects proportional to its power. The result will therefore be a lowering instead of an elevation of the temperature, which lowering may descend to the freezing-point.

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[27] Boudin. Mêmes *Annales*, and p. 43 du tirage à part.

[28] *Histoire de l'Académie des Sciences de Paris*, 1693, t. ii., p. 111.

[29] De la Rive (August). *Traité de l'électricité théorique et applique*, t. ii., p. 247. Walker's trans., vol. ii., p. 303.

It is according to this principle that M. de la Rive explains the thermo-electrical properties of some metals of crystalline molecular structure, in which is sometimes observed, as Pettier first proved, a lowering of temperature by the reunion of the two electricities, as far as the freezing-point of water, and even according to the experiments of Lenz, in five minutes this lowering of temperature reached 4° C. (24° Fahr.).[30]

Seeing that modern physics can supply us with an explanation so satisfactory, we can now readily understand that the congelation of persons or liquids struck by lightning should not be to us a surprising phenomenon. In fact, one has but to suppose that the discharge of the lightning takes place in an *inverse direction* to the polar rotation which the molecules of the body possess for the purpose of originating its own heat; and that then this rotation or motion of heat, having been reversed and diminished, the lowness of the temperature produced may reach congelation. Indeed, heat, properly so called, depends upon a molecular vibration, more intense and in a certain direction; minimum heat, or *cold*, depends also upon this same vibration, but less intense and in an opposite direction.

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[30] De la Rive (August). Work cited, t. ii., p. 242; Walker's translation, vol. ii., p. 305.

Art. IV.—*Superlaryngeal Encysted Tumors; or Encysted Bursal Tumors in Front of the Larynx.* Reported from Dr. F. H. Hamilton's Lecture on Tumors, delivered at Bellevue Hospital Medical College, September, 1869.

There is a small encysted tumor which forms pretty frequently upon the front of the larynx, and which, so far as I know, has not been hitherto described. Of the nine or ten cases which have come under my notice, the position has ranged from a point just above the thyroid cartilage to a point in front of the cricoid cartilage; but in most cases they have been situated directly in front of the crico-thyroid space. Seven of the whole number have occurred in females; several of them commenced in early childhood, or during infancy—possibly some were congenital. They have presented themselves uniformly exactly, or almost exactly, in the median line. They have been globular in form, smooth, elastic, painless, and generally without discoloration. They have seldom caused any inconvenience, unless it has been a slight sensation of constriction. I have never seen them attain a larger size than a small orange; and in most cases they have not been larger than a pullet's egg.

Occasionally they have disappeared somewhat suddenly, but only to reappear after a short period; and in one instance the sac was evidently emptied into the larynx, the contents being coughed up. In one case the tumor disappeared, and had not returned at the end of two years.

The contents of the tumors have, with one exception, been found to be a thin, yellowish serum. In the exceptional case the serum was mixed with blood, the walls being hæmorrhagic. The walls of the cysts have been uniformly thin, and firmly attached to the portions of the larynx over which they were situated.

The surgical treatment which I have adopted has been either incision or excision. Excision is difficult, owing to the thinness of the walls, and to the close attachment of the base to the larynx. Excision has been followed once by sufficient laryngeal irritation to cause some anxiety. Latterly, I have simply laid them open freely, and cut away the projecting portions of the sacs, leaving them to suppurate. When suppuration has failed to destroy the secreting surfaces, they have been made to granulate and cicatrize from the base, by daily injections of the tincture of iodine. In every instance in which the patients have remained under my observation, any length of time, a complete cure has been effected. In regard to any other of those plans of treatment which might naturally suggest themselves, such as the employment of internal remedies, and the application of discutients externally, I have no experience; that is to say, I have not tried them to any extent myself: yet, in most or all of the cases which have come under my notice, therapeutical measures have been employed.

In my opinion these tumors are examples of enlarged bursæ, and I will proceed to state the grounds upon which I base this opinion. In at least one of the examples, and in perhaps two, seen by me, the tumor was situated above the thyroid cartilage, between it and the hyoid bone, where a bursa is usually found. This bursa is not mentioned by Alexander Munro, secundus, in his "Description of all the Bursæ Mucusæ of the Human Body," published in 1770. Velpeau does not allude to it in his great work on the "Surgical Anatomy of the Regions;" but in his "Nouveaux Éléments de Médecine Operatoire," published in 1839, p. 159, vol. iii., he speaks of "the mucous bursa of the thyroid cartilage" as being the possible seat of a sanguinolent effusion. I infer that the reference here made is to the thyro-hyoid bursa.

Grey, in his Anatomy, has properly described the bursa; not, as, according to Horner, limited to the space directly under the centre of the hyoid bone, but as lying between the laminæ of the middle thyro-hyoid ligament, and extending from the hyoid bone to the thyroid cartilage. I have found it of this length both in the infant and in the adult, and generally from three to four lines in breadth.

Most of the examples, however, seen by me, have presented themselves below the thyroid cartilage, in some cases as low as the cricoid cartilage; and since they were, with one exception, globular, I have been unable to regard those situated below the top of the thyroid cartilage as prolongations of the thyro-hyoid bursa. It is possible, indeed, that the fluid of the bursa may have escaped in this direction, and then have been cut off, by adhesion, from the cavity of the bursa, but nothing in the history of these cases has indicated this, and I do not consider the supposition a probable one. They have from the beginning occupied the same, or nearly the same, position as that in which I have found them.

Certainly none of them had any connection with the isthmus of the thyroid gland. They were situated always above the gland—in no case nearer than within half an inch; and in no case has there been a simultaneous enlargement of other portions of this gland. In Case III. the tumor began to form soon after conception, and the thyroid gland is well known to be subject to enlargement under these circumstances; but this tumor was situated at the top of the thyroid cartilage. While, therefore, the thyro-hyoid bursa must explain a portion of these tumors, I am obliged to look for some other explanation of the majority of those which have come under my notice, and I think it may be found in a superficial or subcutaneous bursa, occasionally observed in front of the larynx, caused, probably, by the motion or sliding of the integument over the thyroid and cricoid cartilages—a bursa which may be found, generally small and imperfectly developed, in a certain proportion of bodies.

The following is a brief history of the cases which I have seen:

Case I.—Miss Lang, aged twenty-five, a tumor of the size of a small billiard-ball; had been growing since childhood upon the front of the thyroid cartilage, near the crico-thyroid space. I opened it February 10, 1853, removing a portion of the anterior wall. It was firmly attached to the cartilage, and contained a yellowish, viscid fluid of the consistence of honey. I introduced a tent, and left it to suppurate. Some laryngitis ensued, but in a few weeks it was well.

Case II.—Lucy Gardiner, aged four years. The tumor was first noticed by the mother, the day previous to her consulting me. It was of the size of a large marble, one inch in diameter; round, elastic, and situated directly over the cricoid cartilage. The patient never returned to me, and I have no further history of the case.

Case III.—Lucy Smith, aged twenty, married. June, 1847, soon after conception, she noticed a small tumor on the neck. It gradually increased in size, giving her no inconvenience, however, until it had attained the size of a large marble. September 27, 1848, fourteen months after its first appearance, while stooping to tie her shoes, she felt a sudden "jump" in the tumor, and on looking in the glass she found that it had all disappeared, except a slight elevation. She coughed nothing up. In a few days it filled again, and I dissected out almost the entire sac. It was filled with a light-yellow, gelatinous fluid. The sac was attached to the thyroid cartilage, and to the space above. I saw her several years afterward, and it had never returned.

Case IV.—Niece of Mr. Maxwell, Ellicott Street, Buffalo, New York, aged about twenty-five. The tumor was over the crico-thyroid space, exactly in the median line, about one inch and half in diameter, and perfectly round, elastic, neither tender nor painful. It had existed some years. I have lost my notes of this case, but I remember that she informed me that it once disappeared suddenly, and that she immediately coughed up what she supposed to be its contents. It filled again very soon, and I think it was about the year 1848 that I dissected it out, and found great difficulty in removing its posterior wall, attached to the crico-thyroid ligament, and I believe some portion of it was left. A slight laryngitis ensued after a few days, but in about five or six weeks the wound closed, and she returned to her home in the country perfectly cured.

Case V.—Mrs. P., Brooklyn, called to consult me July 16, 1867, in reference to a small, elastic, smooth, globular tumor, just above the thyroid cartilage, and a very little to the left of the median line; it had existed, more or less, eight or ten years. She had been subject to eruptions from childhood, and had a carbuncle on her face some time since. When the eruptions were present, and when the carbuncle existed, the tumor was smaller; it was once as large as a butternut, and then after a time seemed to disappear entirely. She does not know that it ever evacuated itself into the throat; it causes some inconvenience in deglutition. I advised tincture of iodine, and, if this failed, incision. I met her a year later, and the tumor was gone. It disappeared rather slowly, and she was not using any remedies at the time of its disappearance.

Case VI.—Miss Lida Ridout, of Auburn, aged about twenty-two, consulted me June 18, 1868, with a tumor over the crico-thyroid space, about the size of a pullet's egg; round, smooth, elastic, painless, and situated exactly in the median line. I opened the cyst, and removed as much of its anterior and lateral walls as I could. The contents were a yellow-colored serum. A piece of lint was laid in the wound to encourage suppuration; and after a few weeks it closed, apparently from the bottom. A few months later she returned, with the tumor re-forming. I opened it again, and now continued to inject the sac with tincture of iodine, until it was completely filled up. The skin soon closed over, and the tumor has never returned.

Case VII.—Mrs. Dr. D., New York City, aged about forty, called my attention in January, 1868, to a tumor exactly over the crico-thyroid space, of the size of a pullet's egg; round, smooth, and painless. She had discovered it three years before. She thought it sometimes diminished in size, and then again enlarged. It gave her no inconvenience, and she did not desire to adopt any treatment for its relief.

Case VIII.—April 5, 1856, I was consulted by the parents of Daniel Lockwood, aged eleven, in reference to a tumor which commenced upon his neck three years before. It had disappeared five or six times during this period, never suddenly, but each time gradually. The last two times of its reappearance the surface had looked reddish. When seen by me it was oblong, elastic, red, and tender; one inch long by half an inch in width; the longest diameter being vertical. The father informed me that it was now a little lower down than formerly, being at this time situated over the crico-thyroid space and cricoid cartilage.

I advised that the tumor should be opened freely at once, but he preferred to wait and try what might be accomplished by discutients. I never saw him again, and do not know the result.

Case IX.—The following case was no doubt of the same character, although its exact situation is not indicated in my notes of the case:

September 25, 1855, Albert Blackman, of Marilla, Erie County, N. Y., aged twenty-three, consulted me in relation to a tumor, of the size of a Sicily orange (a small orange), globular, smooth, elastic, very little to the right of centre of the neck. "Interferes some with breathing. Has to eat slowly. Commenced in childhood; when first noticed, it was of about the size of a pea, and exactly in the centre."

I opened it and evacuated about six ounces of amber-colored, thin serum; the color was due to the presence of blood; the inner wall of the sac was evidently hæmorrhagic, as it filled again rapidly with blood. I introduced a tent and applied strips of adhesive plaster, with the view of promoting suppuration. He returned to his home the same day, and I did not see him again.

Case X.—Mr. Lyons, of Geneva, consulted me January 4, 1844, in reference to his son, five years old, who had a fistulous discharge from the front of the lower part of the larynx, which had existed since he was one year old. The fluid discharged was thin and glairy. A probe could be passed upward, beneath the integument to the top of the thyroid cartilage.

I recommended that the fistulous canal should be injected with the tincture of iodine daily. Some improvement resulted from this treatment, but I lost sight of the case after a few weeks, and before a cure was effected.

This case has been referred to in the preceding remarks, as an example of the disease described by Physic, and as having probably connection with the thyro-hyoid bursa.

In connection with these cases I take the liberty of appending the following extract from a note from Dr. J. H. Pooley, of Yonkers, describing a case which had come under his observation:

> "At your request, I send you the following note of the case of congenital fistula of the neck, which I alluded to at the last meeting of the New York Journal Association.
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> "I attended Mrs. C., in labor with her first child, and when the child was born, my attention was directed to what looked like an abrasion of the skin in front of the neck, and at first thought I might have inadvertently scratched it with my nail. Upon examination it proved to be the opening of a little sinus or fistula, situated in the median line over the thyro-hyoid space, passing somewhat obliquely backward and downward into the tissues of the neck, for about half an inch, and just large enough to admit the ordinary-sized probe of a pocket-case. No communication with the trachea could be found, though carefully sought for. The opening of the little fistulous canal had the ordinary mucous appearance of such orifices. Just above it was a similar little fistula, but not so deep, barely an eighth of an inch. No discharge either of fluid or air was ever observed to issue from either of them. The child died when only two months old, of intestinal obstruction, but no *post-mortem* examination could be obtained. I thought, until hearing your paper the other night, that this must be a case of those fistulous communications with the trachea, the internal opening of which had closed; but, since hearing your case of a superlaryngeal bursal tumor leading to fistulous openings, I have questioned whether this may not have had a similar origin: if I had entertained this suspicion at the time, probably my notes would have furnished some additional particulars in elucidation of this question."

ART. V.—*Notes on the History of Relapsing Fever.* By MEREDITH CLYMER, M. D.

HAVING observed and reported the first cases of Relapsing Fever which happened in the United States, twenty-five years ago,[1] I was naturally anxious to see and establish the identity or non-identity of the form of fever now prevalent in this city, and styled relapsing, with my cases of 1844. Through the kindness of Dr. Elisha Harris, Sanitary Superintendent of the Metropolitan Board of Health, and the medical officers of Bellevue Hospital, ample opportunities have been given me to do this. Although several of the symptoms prominent in the Philadelphia cases of 1844, notably the peculiar bronze hue of the skin, are absent, I am satisfied of the identity of the diseases. The New York cases will be reported by competent observers, and I will only here mention some of the points connected with the first appearance of relapsing fever in the United States, and give a sketch of its European history.

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[1] Fevers: their Diagnosis, Pathology, and Treatment. By Meredith Clymer, M. D. Philadelphia, 1845, pp. 99.

In June, 1844, a Liverpool packet with Irish emigrants arrived at Philadelphia. There had been some sickness on board during the voyage, and fifteen of the steerage passengers were immediately sent to the Philadelphia Hospital, and put in the wards in my charge. They were ill with a form of continued fever unlike any that I was then familiar with. I thought that I recognized (by the descriptions I had read of it) a variety of fever which had prevailed in Scotland and parts of England, during that and the previous year (1843–'44). The event proved the correctness of my conjecture. In all these cases the access was sudden, with severe headache, vomiting, occasional epistaxis, muscular and joint pains, rapid pulse, high body-heat, tongue coated with a thick creamy-gray or yellow fur, with red edges and tip (in some cases, for several days quite dry, as was the mucous membrane of the mouth), enlarged spleen, no special intestinal symptoms, and no eruption. In all there were great debility, and a peculiar bronzed hue of the face, more or less like that of persons suffering from chronic malarial toxæmia.[1] About the seventh day the febrile symptoms suddenly subsided, with copious sweatings; the appetite became good, the expression of countenance natural, but much muscular weakness persisted, and the color of the face was of a dull straw chloro-anæmic tint. In every case about the fourteenth day a relapse happened, which in some had all the severity of the initial paroxysm, while in others it was slight. So far as I know, there was no second relapse; all were discharged from hospital during their second convalescence, at their own request, though many were still quite feeble. It was perhaps for this reason that I saw none of the sequelæ, reported so common after the disorder in Great Britain, as ophthalmia, with amaurotic symptoms, boils, swellings of the legs and ankles, pain in the feet without swelling, paralysis of the deltoid and other muscles. The disease did not extend to the nurses or other inmates of the hospital, though the fever-patients were not segregated. Two sisters, however, who had been residents of the city for several years, but whose brother had come out in the same ship and been taken ill at their house a few days after his arrival, were admitted into the wards with well-marked attacks of the fever. Several other passengers were at this time in the hospital, complaining of slight chills, and sweatings, headache, nausea, and vomiting, with loss of strength, but got well without any other decided symptoms of the disease.

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[1] An eminent physician of Savannah, who saw these cases in my wards, remarked that they had very much the physiognomy of persons with the seasoning fever of the South. Dr. Rose Cormack indeed suggested (Edinburgh, 1844) that there was an analogy, if not identity, between this fever and the malarious form of yellow fever: a resemblance, however, which observation and the history of the two disorders do not make possible.

The treatment consisted of mild salines, a nourishing diet, with quinine and wine.

Several cases of illness from the same ship were sent to the Pennsylvania Hospital, and were under the care of the late Prof. W. Pepper, who at first was disposed to regard them as mild typhus, but he subsequently admitted their resemblance to the British epidemic.

Prof. Austin Flint (" Principles and Practice of Medicine ") says that Dr. A. Dubois, of New York, reported in the " Transactions of the American Medical Association," vol. ii., a few cases in 1848. In the *first* volume of the Transactions of the Association, and in *The Annalist*, June, 1848, Dr. Dubois has an article entitled " Ophthalmia post Febrilis ; a Severe Form of Inflammation of the Eye, following Typhus Fever as it appeared in the City of New York in 1847–'48." From the ocular sequelæ he infers that it might have been the same type of fever which prevailed in Dublin, 1826, and in Edinburgh, 1843 ; he does not say that he saw any cases of the fever, nor does he give the clinical history of any ; he publishes a short note from the late Dr. Swett, at the time one of the physicians of the New York Hospital, with a summary of the symptoms of the cases of fever which had been admitted into the hospital during the years 1867–'68, and certainly no one can recognize the phenomena or course of true relapsing fever in Dr. Swett's description. "The duration of the disease was from two to three weeks," and not from five to seven days; "the condition of the brain and nervous system did not differ from the fever of former years;" diarrhœa was of frequent occurrence; there were inflammatory complications, which varied with the season, as dysentery in the autumn, peritonitis in the winter, pleurisy in the spring, etc. Bronchitis was common, with occasional "inflammation of the fauces, tending to œdema of the glottis." In "many of the cases" there was "an abundant rose-colored rash on the trunks and limbs," which came "in patches irregular in shape and size, disappearing on pressure." There was no constant lesion; sometimes the glands of Peyer were affected, and sometimes there was ulceration of the large intestines. Relapses were by no means infrequent. I have been permitted to examine the medical case-books of the hospital for these years, and I can say that, from the reports there entered of the fever cases, there was no analogy nor identity between them and true relapsing fever.

Relapsing fever seems to have occurred from time to time in the British Isles, since the middle of the last century, and since 1846 to have occasionally been met with on the continent of Europe. It has been variously described under the names of *five or seven day fever*, *seventeen-day fever*, *bilious-relapsing fever*, *short-relapsing fever*, *short fever*, *break-bone fever*, *famine fever*, *hunger fever*, or *hunger pest*, *synocha*, *typhus recurrens*, *etc.*

By Rutty's "Chronological History of the Weather, Season, and Diseases in Dublin, from 1725 to 1765," London, 1771, a similar epidemic appears to have prevailed during July, August, September, and October, 1739, and again in 1741. Of the first he says: "It terminated sometimes in four, for the most part in five or six days, and commonly in a critical sweat: it was far from being mortal." Of that of 1741, he writes: "Through the three summer months there was frequent here and there a fever, altogether without the malignity attending the former, of six or seven days' duration, terminating in a critical sweat; but in this the patients were more subject to a relapse, even to a third or fourth time, and yet recovered." It undoubtedly preceded and accompanied the great typhus epidemics of 1816, '17, '18, '19 (Barker and Cheyne), and 1826 (Reid and O'Brien), in Ireland. Dr. O'Brien says that, at the beginning, there were two fevers, the "ordinary typhus or fever of the old constitution, which was very fatal," and "a fever of the new constitution, lasting only a few days, and seldom fatal, but frequently relapsing." At first the relapsing cases were predominant, but, as the epidemic advanced, the proportion of relapsing cases greatly decreased. From 1828 to 1842 it may be said to have disappeared from Britain (Murchison). It was a common form of fever in Edinburgh from 1826 to 1829; and in 1843–'44 it again appeared in Scotland and parts of England (Rose Cormack, Henderson, etc.). In 1847 it became epidemic in Edinburgh, Glasgow, and other large manufacturing towns of Scotland (Waters, Orr, R. Paterson). In the same year it was prevalent also in London, and was described by Ormerod, Bottomley, and Sir William Jenner; the latter, in his memoir, remarks on the constancy of its characters since it was first known (*Medical Times*, 1849–'51). It no doubt formed the great bulk of the cases in the fever epidemic which raged in parts of Germany, particularly Upper Silesia, in 1847, as described by Virchow, Von Bärensprung, Dümler, and Suchanek. It is true that it was not generally recognized as such at the time by the German writers, who made no distinction between it and typhus and typhoid fevers; but Dr. Parkes shows ("Diagnosis of Fevers") that, from the case-histories, these epidemics were mainly made up of relapsing fever. In 1855 it broke out among the British troops in the Crimea (Lyons).

Dr. Baldon has published an account of an epidemic of relapsing fever, which appeared in Peru in 1854, and proceeded along the chain of the Andes, but never at a lower elevation than 1,500 metres. In 1859 it reached Bolivia, and the neighboring parts of Chili. In 1,000 cases the deaths were 250, about equal in both sexes—children, under sixteen years, 92; adults, 110; over sixty years, 48.

Toward the end of 1864, and during the first part of 1865, relapsing fever made its appearance at St. Petersburg, and, not being at first recognized, it seems to have caused much alarm. In a report made to the Medical Officer of the Privy Council by Dr. George Whitley, who had been sent to the Russian capital with instructions to inquire into the kind of fever which had been recently epidemic there, it is stated that, in its rise, progress, causes, symptoms, and fatality, it resembled the British relapsing fever, and was of a form until then unknown to the physicians of St. Petersburg. The number of cases of fever admitted into the civil and military hospitals from the outbreak, August, 1864, to March 1, 1865, were 7,097 typhus, and 7,625 relapsing fever; the deaths from typhus were 1,198, and from relapsing fever 836; under the head of typhus, both typhus and typhoid fever are included. Dubowitski, in "A Report to the English Ambassador by the Russian Government," says that, though it had never before visited St. Petersburg, it had been at Moscow in 1840, and at New Archangel, in Russian America, in 1858. In 1863, '64, '65, it prevailed at Odessa (Bernstein).

During 1867 it appeared in East Prussia, having probably extended westward from Russia; and we find in the German medical periodicals of 1868 very full and accurate accounts of it, under the name of *typhus recurrens*. Riess gave a history of it when at Berlin (*Berlin. Klin. Wochenschrift*, No. 22, 1868); Swidersky described its visitation of Posen (*Deutsche Klinik*, No. 51, 1868); and Lebert and Pastau published clinical histories, and the results of their observation of the disease at Breslau (*Berlin. Klin. Wochenschrift*, No. 26, 1868; *Bul. de l'Académie de Méd.*, xxxiii., 821; and *Berlin. Klin. Wochenschr.*, No. 40, 1868). During the same year some cases are reported in Belgium (Van Biervliet fils; *la Presse Méd. Belge*, 7 and 8, 1868).

Since the epidemic of 1847–'48, relapsing fever, Dr. Murchison says, has been gradually disappearing from Great Britain, and that for twelve years previous to 1868 not one case had been seen in the hospitals of London, Edinburgh, and Glasgow. Prof. W. T. Gairdner had not seen or heard (1868) of a single case at Edinburgh since 1855; and, according to Dr. Lyons, true relapsing fever has been a rare disease in Ireland of late years.

In the summer and early autumn of 1869 relapsing fever threatened to become again epidemic in London. On July 4, 1868, an Irish girl, aged twenty, eight years resident of London, was admitted into the Fever Hospital from Whitechapel, with the disease. This was the first case in that institution for fourteen years. Four days later, a Polish Jewess, not speaking English, and whose length of residence in London could not be ascertained, was received, also from Whitechapel; and on July 30th a third case, a girl, aged fourteen, who had lived in London all her life, was admitted. Several other patients with the fever, including a Polish family, came into the hospital between the 1st and 6th of July. In September and December, 1868, seven Polish Jews were admitted into the German Hospital (Weber, *Medical Times and Gazette*, December 19, 1869, and *The Lancet*, February, 1869). As has been stated above, relapsing fever was prevailing in Prussia at this time, and there is no evidence to show that there were any cases in Ireland and Scotland, as in former epidemics; and though neither at the Fever nor German Hospital could the first cases be traced directly to importation, still, as Dr. Murchison remarks, circumstances seem to point to the probability of the disease having been brought in the summer of 1868 from Germany.

In May, 1869, the disease again appeared at the Fever Hospital, and, during the months of September, October, and November, the number of cases rapidly increased, and it was apparent that it had become epidemic. The admissions were: May, 1869, 4 cases; June, 3 cases; July, 7 cases; August, 15 cases; September, 34 cases; October, 127 cases; November (first fortnight), 104 cases. Of the 70 cases received up to October 1st, inclusive, 2 were contracted in the hospital, and the remaining 68 were brought from 19 of the 36 parochial districts of London; but Whitechapel, which furnished the first cases to the German and Fever Hospitals, and Bethnal Green, together supplied more than one-half the cases. None of the patients, however, came from the same houses or even streets as the patients admitted into the two hospitals in the summer and autumn of 1868. None of the sixty-eight, we are told, were natives of Germany, and nearly all had resided in London for many years; with few exceptions, they were in a deplorable state of destitution.

The disease presents all the characteristic features observed in former epidemics. Dr. Murchison (*Lancet*, vol. ii., 1869, p. 504) thus summarizes the symptoms: "The pyrexia sets in suddenly, the temperature often rising within the first twelve hours to 104° or 105° Fahr., and the pulse to 120 or higher, accompanied by severe headache, and muscular and arthritic pains, sleeplessness, but no impairment of intellect, painful enlargement of the liver and spleen, in many instances frequent and distressing vomiting, and in some slight jaundice. These symptoms subside suddenly with profuse perspiration, about the fifth, or, oftener, on the seventh day. In a few hours the pulse may fall from 128 to 84 or 60, and the temperature from 104° or 105° Fahr. to 96° Fahr." In a case I saw in this city January 30th, at 10.30 A. M., the pulse, which on the preceding evening had been 115, was 66, and the body-heat had fallen from 107° to 98°.5 Fahr. Dr. Murchison reports a case (*The Lancet*, *l. c.*), in which, on the evening of December 12, 1869, the pulse was 108, and the temperature 104° Fahr.; on the next morning the pulse was 75, and the temperature 96°.6 Fahr. A little sickness and hepatic tenderness may remain for a day or two, but, as a rule, the patient at once regains his appetite, and may be up and wishing to leave the hospital, when on the fourteenth day from his first attack there is an abrupt relapse of all his former symptoms, lasting for three or four days, and then subsiding.

Rashes, though not constant, have been noticed; in some a sub-cuticular mottling, not unlike that in typhus, in others rose-colored spots of typhoid, while many had small hæmorrhagic dots about the size of a pin's head. There were no true petechiæ. Crops of herpetic vesicles on the face appeared in several. In a clinical lecture (*The Lancet*, January 22, 1870), Dr. Murchison says: "There is no specific eruption, the innumerable small petechiæ like flea-bites, except that they are a little larger, are really the results of insect-bites, or small purpura spots, from an altered condition of the blood, and are in no wise specific." Zuelzer, in his account of the St. Petersburg epidemic in the Appendix to his translation of Murchison on Continued Fevers, states that in some cases the chest, abdomen, and back, were covered with numerous small red spots, disappearing on pressure, and lasting for three or four days. Wyss and Bock, in their account of the fever at Breslau, speak of the eruption as similar to that of exanthematic typhus. In the case given by Dr. Murchison in this lecture, the front of the chest and abdomen were covered by a number of minute lake-colored spots, not at all elevated, and disappearing on pressure. He remarks that he has met with the same eruption five times only in several hundred cases of the present London epidemic which he had seen, but that it is very exceptional. In one of the New York cases which I saw, there was a regular maculated eruption in its several stages.

Quinine in twenty-grain doses had no effect in preventing relapses or modifying the course of the disease in the London cases. The same want of all controlling power of the drug was seen, Dr. Whitley says, in the St. Petersburg cases.

It has been remarked by systematic writers that relapsing fever has never been met with in India or in warm climates; but it seems that without doubt famine fever prevailed to such an extent in the Punjaub in the autumn of 1868, among the muleteers who had returned from the Abyssinian expedition, that it became necessary to enforce a strict quarantine (*The Lancet*, vol. ii., 1869). An account of an epidemic of relapsing fever at the factory on the river Dumas, *Ile de la Réunion*, in 1865, has been published by Dr. MacAuliffe, in the *Archives de Médecine Navale*, 1868.

On the 23d of October, 1869, Mr. John Simon, the distinguished chief of the Medical Department of the Privy Council Office, issued a circular, in which, after alluding to the probable importation of the disease into London, and sketching the natural history of the fever, he gives a series of practical hints to those who have to deal with the disorder in the way of prevention. He writes: "The greatest personal predisposition is given by states of poverty and privation. It is in a very high degree communicable from sick to healthy. It is eminently a disease that cannot safely be treated in the houses of the poor."

This circular has probably led to the building of a temporary hospital at Hampstead, London, at a cost of £5,542, which has just been opened. In *The Lancet*, January 22, 1870, Dr. Murchison says: "The disease has spread so rapidly that the resources of this vast metropolis are now being taxed to their utmost to find accommodation for persons attacked with it."

The disease is more painful than typhus, but its mortality is, as a rule, small; but, as its victims are left for a long time in a very feeble state, they are in a peculiarly favorable condition to contract typhus fever, an epidemic of which seems invariably to accompany famine fever.

The conditions favorable to an outbreak of relapsing fever are essentially those of destitution and starvation. It is emphatically a famine fever. The pauper population are almost its only victims. In the Edinburgh epidemic of 1843–'44, we are told that the disease was confined to the poor, and that the medical men whose practice was among the better class did not see a case. In Glasgow, for two years before its appearance, the poor had been in a state of extreme privation. In Leith, "great misery and destitution prevailed among the poor." Of 1,768 cases collected by Allison, Halliday, Douglas, and Murray, 1,179 (about two-thirds) were out of employment and utterly destitute at the time of seizure. Dr. Lynch, of Loughrea, reported (1848), "most of the cases of fever supervening upon the starvation-state were characterized by repeated relapses and short febrile attacks. I saw no instances of the short relapse-fever among the gentry, except in clergymen and physicians." Similar observations were made in London. Dr. Whitley says that there was much suffering among the poor at the time of its outbreak at St. Petersburg, and that their chief food, head-cabbage and fish, was scarce and inferior in quality, and vegetables generally had been destroyed by an early frost. The editor of the *Medical Times and Gazette* writes concerning the present London epidemic (November 6, 1869): "The price of provisions has been inordinately high. Wheat prior to the last harvest averaged 72 s., and meat is an article which many of the poor have been unable to obtain at all. Butchers, carrying on their trade in the less wealthy quarters of London, assure us that the poor, who formerly bought little, now do not buy at all." Dr. Murchison says of the patients admitted into the Fever Hospital with the disorder: "With few exceptions, all have been in a deplorable state of destitution." Mr. Simon observes: "Where destitution has not existed, relapsing fever is not likely to be epidemic." The Germans have recognized the connection between ill-feeding and this fever, by the name they have given it—*hunger-pest.* Overcrowding, so potent an ætic factor in typhus, seems to have but little part in the causation of relapsing fever. Weather has no influence over its origin or propagation. "It prevails alike in seasons remarkable for the amount of rain (Silesia, 1847), and in seasons remarkable for their drought (Edinburgh, 1843); in unusually hot summers (Edinburgh, 1843), and in the cold of winter (Glasgow, 1842–'43, and Leith, 1843–'44)," (Murchison, p. 314). There is every reason to believe that the poisons of typhus and relapsing fever are distinct, that the one fever never communicates the other, and that an attack of the one gives no immunity from an attack of the other (Henderson), though they often prevail conjointly.

Relapsing fever is intensely infectious, and its cases require peremptory isolation.

Since the above was written, and in print, Dr. Harris, the Sanitary Superintendent of the Metropolitan Board of Health, has kindly given me the following facts concerning the progress of the present epidemic of the fever in this city:

Thirty-eight groups, or fever-nests, including one hundred and ninety-three cases; of these, one hundred and sixty-four cases have been thoroughly studied, and found to have been limited in origin to thirty-five tenements, which usually consisted of a single living-room, with a communicating bedroom off. In only one instance it was found in two floors of the same house. Four of the groups were on the top-floor, three on a middle-floor, and all the others on first-floor, basement, or cellar—cellars greatly preponderating. With one exception all the groups have been traced to five, and possibly to three primary points of departure. One of the first groups will illustrate the history of the whole:

No. 59 Cherry Street, cellar lodging-house. In the last week of December, sickness was reported there. Sanitary Inspector O. G. Smith, of that District, examined the patients, and expressed the opinion that it was typhus or typhoid, and recommended that all the sick be immediately removed to hospital. He ascertained that various lodgers from that cellar had entered hospital at different times during the preceding six weeks. The cellar was summarily evacuated, under official orders, January 3d, all the sick being sent to hospital, and the family and lodgers catalogued, and put under observation in their new home above-ground. Now this is the record: 1. No less than nine persons had suffered from relapsing fever in that cellar previous to January 1st. 2. These patients had bowel symptoms, and one at least had eruption which was pronounced petechial. At this cellar, No. 59, two children named Higgins, from 73, in the same block, and on the first floor, in comparatively well-ventilated quarters, passed in and out, and became sick early in January. Three cases occurred in one room at the latter tenement, and several others are believed to be traceable to the same place. 3. Just before the Board of Health caused No. 59 to be evacuated, a mother and daughter, named Casey, transferred their lodging to 79 James Street, where each of them in succession became sick and went to hospital; and, a little more than a week after the daughter went, the keeper of the lodging-house, Mrs. Dunn, became sick. This cellar was summarily evacuated, and the inmates placed in new quarters above-ground, and put under observationf or further developments. 4. At the cellar in 59 Cherry Street, two seamen, named Powers and Harris, were frequent visitors during the month of December. Powers became very sick, and was sent from the forecastle directly to the Seamen's Hospital; while the deck-hand, Harris, became so sick on the 26th of December that it was necessary to take him from his forecastle lodging at the foot of Catharine Street, and convey him to the Shailor's Boarding-House, at 332 Water Street, where he was laid in a close, dark lodging-room, with three other lodgers, all of whom became sick with the same fever in less than two weeks, this man being sent to hospital after two or three days. 5. Another seaman, by the name of Thomas Kane, from the same forecastle, and who was also a fellow-lodger with the seaman Harris, on the 27th of December, at the lodging-house above-mentioned, had been sick with the fever from Christmas. On the 7th of January this man came, feeling unable to return to his duties on shipboard, visited his sister, Mrs. O'Brien, living on the top-floor of front-house, 337 First Avenue; became very sick, with high fever, that night. His sister's family consisted of her husband and herself, an infant at the breast, and four little children at various ages. The children lay down beside their sick uncle, and slept all night under the same blanket upon the floor. These children and the entire family have sickened successively in the following order, viz.: (1.) The four children that slept at the side of their sick uncle on the night of the 7th of January, the period of incubation in this case being precisely nine days. (2.) Mrs. O'Brien and her infant; and (3d.) In this whole group of five children and two adults, Mrs. O'Brien became sick on the night of Sunday, the 5th of February.

Proceedings of Societies.

MEDICAL SOCIETY OF THE COUNTY OF NEW YORK.

Adjourned Stated Meeting, January 17, 1870.

DR. GEORGE T. ELLIOT, President, in the Chair.

SPINAL IRRITATION.

DR. WILLIAM A. HAMMOND read a very valuable paper upon *Spinal Irritation*, which will appear in the PSYCHOLOGICAL JOURNAL for April, and should be read by every physician. We give, in brief, some of its chief points :

After alluding to the distrust experienced by several distinguished writers, and shared by a large part of the profession, as to the existence of an independent affection which may properly be called "spinal irritation," the speaker stated emphatically his own conviction that there is such a disorder of the spinal cord, by no means rare or unimportant, and no more to be confounded with hysteria or the various other affections which it may simulate, and to which its symptoms have so often been referred, than with organic disease of the cord. He carefully reviewed the literature of the subject, giving a *résumé* of the more important facts and opinions advanced concerning it, from the introduction of the term by Dr. C. Brown, of Glasgow, in 1828, down to the present time. He then gave his own views, based upon a study of one hundred and twelve cases occurring in his private practice, eighty-three of which had been fully recorded, and twenty-nine less completely.

Symptoms.—(*a*) Centric symptoms: 1. Tenderness at one or more points over the spinal column, increased by pressure. This symptom Dr. H. regards as invariably present, although sometimes developed only by careful examination, and occasionally appearing only several moments after the pressure is applied. Any case which does not exhibit it he excludes from the category of spinal irritation. Dr. Austin Flint and some others think it may be absent in exceptional cases. The tenderness varies in character from a dull ache, seated in the deeper tissues and developed by strong pressure, to a lancinating pain, seated in the skin and subcutaneous areolar tissue, and excited by slight pressure. It varies in degree from a slight discomfort to a hyperæsthesia rendering the touch of the clothing insufferable. It may be limited in extent to the spot under pressure, or the pain may be propagated along the spinal nerves. The seat of the tenderness is most frequently the dorsal region, but may be the cervical or the lumbar, and it may extend over the whole spine. Each location has its characteristic eccentric symptoms. 2. Pain in the cord. The tenderness above described was external to the vertebral canal. The pain now spoken of is in the cord itself, and therefore cannot be excited (unless in a reflex way, through the former) by pressure on the spinous processes; but it may be excited by percussion and by motion of the spinal column. The pain is commonly felt near the point of external tenderness, but may be distant from it. It was present in one hundred and nine of Dr. Hammond's cases.

(*b*) Eccentric symptoms. These constitute the most noticeable ones, and vary in accordance with the part of the cord irritated. Among those occurring in Dr. Hammond's cases are the following, under their respective regions: 1. *Cervical irritation:* Vertigo, headache, tinnitus aurium, visual disturbance, sense of frontal constriction; tenderness of scalp; mental aberration (more or less marked in every case); insomnia, or excessive somnolence; neuralgic pains and motor disturbance in parts deriving their nervous supply from the affected region—in scalp and face, if this were the upper cervical; in upper part of chest and upper extremities, if it were the lower cervical; nausea and vomiting, but *not* gastric pain. 2. *Dorsal irritation:* Gastralgia (in every case), gastric flatulence, acidity, nausea and vomiting, pyrosis; palpitation, cardiac oppression, syncope; dyspnœa, cough; intercostal neuralgia, inframammary pain (very frequent); motor disorder (spasm or paralysis). 3. *Lumbar irritation:* Neuralgia of lower extremities, and sometimes of back and abdomen; uterine, ovarian, and rectal pain; strangury; tonic spasm of muscles in lower extremities, clonic spasms (occasional in every case), paralysis. The above symptoms are taken from cases where the tenderness was located in but a single region of the spine. In those where it was located in both the cervical and the dorsal region, or both the dorsal and the lumbar, the symptoms presented a combination of those characteristic of each region ; and in ten cases, where the whole spine was tender, they were quite irregular in their manifestations from time to time.

Causes.—Sex is the strongest predisposing cause, ninety-three of the one hundred and twelve cases being females. Age has its effect, fifty of eighty-three cases being between fifteen and twenty-five years. Hereditary influence was ascertained in some instances. The exciting causes are often impossible to fix. Among those determined in the cases reported, are mechanical violence, sexual excesses, mental fatigue and anxiety, innutrition, abuse of alcohol and of opium, exhausting diseases.

Pathology.—The essential pathological condition in this affection Dr. Hammond considers to be anæmia of the cord, and he gives *in extenso* his reasons for this opinion. Other writers have attributed the spinal irritation to congestion, inflammation, and many other conditions. That anæmia may cause the irritability is shown by analogy, and that local anæmias may be produced is explicable with our present knowledge of the vaso-motor function of the sympathetic. The irritation thus established would seem also to have a secondary influence upon the sympathetic, resulting in the visceral disturbances that constitute so important a feature of the disease. The well-known laws of reflex action suffice to explain the effects of pressure, percussion, etc., and the aberrations of sensation and motility.

The *diagnosis*, after rejecting all cases which fail to present the vertebral tenderness, lies between this affection and the other spinal diseases which, in their earlier stages, may resemble it—chronic myelitis, meningitis, and congestion. An early and correct diagnosis is of the greatest moment as a guide to treatment, the indications for which, in spinal irritation, are quite the reverse of those in the other affections. In a matter of such vital consequence we shall not attempt the imperfect abstract our limits would compel, but refer the reader to the paper itself.

The *prognosis* is favorable, all cases being alleviated by persistent treatment, and nearly all being ultimately cured.

Treatment.—The indications are four: "1. To remove the cause; 2. To improve the general tone of the system; 3. To increase the amount of blood in the spinal cord, and improve the nutrition of this organ; 4. To set up a counter-irritant action in the vicinity of the disordered region of the cord." The first indication speaks for itself. The second is met by tonics (as iron, quinine, zinc, cod-liver oil), and especially by alcoholic stimulants. The third by strychnia, phosphorus, phosphoric acid, opium, heat to the spine, the recumbent posture, and, above all, the direct galvanic current scientifically applied. (The induced current also is of service applied to the affected muscles, where paralysis is present.) Of counter-irritants, blisters and dry cups are to be preferred to antimonial ointment. Wet cups or leeches are inadmissible.

The paper concluded by reports of illustrative cases.

Dr. Chadsey had, in his early practice, seen much of what he had considered to be spinal irritation, occurring chiefly among a hardy, laboring population in a northern county. He had recorded upward of one hundred cases. His treatment had been heroically antiphlogistic, with antimonial ointment to the spine, and had given satisfactory results. In three fatal cases *post-mortem* examination had shown congestion of the meninges of the cord under the points of tenderness. Therefore, while recognizing the excellence of Dr. Hammond's description of the disease, he could not agree with his pathology or his treatment. He could not see how blows and strains, which had been mentioned among the exciting causes, could produce spinal anæmia; and, if the morbid condition were, as he deemed it, one of hyperæmia tending to inflammation, then tonics were the last thing to be given, and local bloodletting should prove of the greatest service. Electricity he had found of no use at the onset of the disease, but very useful after local depletion. In corroboration of his remarks he related several cases.

Dr. Fordyce Barker.—There is fashion in diseases as in dress. The most of us will recollect that, twenty-five years ago, spinal irritation was exceedingly common, but of late it has gone so completely out of vogue that, when I saw the subject of the paper announced, I thought it demanded a good deal of moral courage to present it. A few years since, when I gave the diagnosis of spinal irritation, in a case to which I had been called in consultation, the attending physician replied: "Oh, yes, that does very well to tell the family, but what do you really think of the case? For 'spinal irritation,' you know, is only a phrase to cover ignorance." But I do believe in it, having often met with it pure and simple, as well as often complicating other affections; and the paper of to-night I regard as especially timely in recalling attention to a class of diseases too often misunderstood, if not utterly ignored.

Dr. Hammond has given us a very clear diagnosis between spinal irritation and organic diseases of the cord. I wish he had gone further and shown how to distinguish it from hysteria, chorea, and nervous asthenia. Where spinal irritation was associated with one or the other of these disorders, I have sometimes found it very difficult to decide which was the primary affection. But the results of treatment may often help us. I was consulted by a lady who had been subject to chorea of a severe character for a couple of years, and had been under most excellent treatment for that disease, but without gaining relief. I found dorsal and cervical tenderness, and directed treatment to the spinal irritation, when that for the chorea became effective. Another lady, the mother of three children, had been unable for a year and a half to carry the food to her mouth. The slightest cause, such as an unexpected noise, would throw her into terrible choreic and hysterical convulsions. Spinal irritation was diagnosticated, and its treatment had the same happy result as in the former case. I have met with several instances where spinal irritation manifested itself especially in connection with the chest. In one which I recall, a lady was harassed by a peculiar cough, sounding like the bark of a small dog, and absolutely incessant day and night. It was, of course, very distressing, and when I saw the patient she was in a state of great nervous prostration. Dr. Horace Green had removed a portion of the tonsil with slight temporary relief; other treatment in great variety had been instituted, but the cough still continued.

But, when I treated her for spinal irritation, a perfect cure was the result.

Among the organic diseases which, like hysteria, spinal irritation is apt to simulate, I have found uterine diseases the most frequent ; next, those of the digestive system, and then those of the respiratory. I have never seen it counterfeiting organic disease of the heart or brain. This relative frequency may probably depend upon the class of practice to which I confine myself. Every year I meet three or four cases of this kind : A woman has been under treatment for uterine disease —treatment which I know has been perfectly adapted to such disease, if she had it. Upon examination I find that the uterine disease either has been cured or has never existed ; but the symptoms continue. I find general exhaustion and depression, with tenderness over the lumbar vertebræ ; and, treating the case accordingly, I find the treatment successful. I was called by the mother to see a boarding-school miss of sixteen, supposed to have some uterine affection. She suffered from pain in the back, irregular menstruation, dysmenorrhœa, with bearing-down pains, headache, loss of flesh, loss of interest in life, etc. I made up my mind that the case was one of spinal irritation, and, on proceeding to direct the treatment, found that I had quite lost the mother's confidence by not proposing a vaginal examination. The young lady, she said, had been schooled to meet the occasion, and was fully prepared for any thing that might be necessary. I left her still distrustful, but am happy to say that her confidence was restored by the effect of the treatment.

I was surprised at the great preponderance of females among Dr. Hammond's cases, although this is quite as great in my own practice. But I have seen as striking examples of the affection in men as in women—one in particular. A gentleman had been engaged in active business pursuits, partly in this city and partly in London, until the age of forty-five, when, with a large fortune, he retired from business, residing here, and living generously as a gentleman of leisure. He was a vigorous, hearty man, weighing one hundred and eighty pounds, and when, some three years after, he had an attack of pneumonia, he told me it was the first day's illness and almost the first pain he had ever experienced. At the age of fifty he married, and I lost sight of him. A few years later, after a residence in Europe, he came to me a complete hypochondriac, whom I should never have recognized as my old acquaintance. For nearly two years he had been falling away, till he weighed but one hundred and twenty pounds. He had no appetite, constant nausea, headache, and sleeplessness. He had lost all interest in society, in reading, in the company of his friends, even in his money, and was the victim of chronic melancholia, with a tendency to suicide. Among the many physicians he had consulted, was a friend of mine in London, especially eminent as an author on digestive disorders. After six weeks' observation of the patient, this gentleman sent him to me, with a letter expressing his conviction that the case was one of malignant disease of the stomach. The man had been put upon the blandest diet, and charged to cut off all stimulants, although he had been used to free living. Under this regimen he had been getting worse and worse. A most careful examination gave me no conclusive evidence of organic disease of the stomach, of any kind; but it did disclose excessive tenderness and constant pain in the lower cervical and upper dorsal regions of the spine. I commenced counter-irritation over those parts; compelled the patient to take strong beef-tea, and other nutriment; gave tonics and stimulants. In four months he had gained over forty pounds, and he is now in perfect health. During the great financial fluctuations incident to the late war, I quite frequently met with this affection among the speculators of Wall Street, who taxed their strength to its utmost by the feverish excitement of their life, sustaining themselves by drinking largely, without intoxication, until the inevitable reaction came.

In the general principles of treatment enunciated by Dr. Hammond I fully concur. With regard to matters of detail, I may venture to add a few suggestions. The high estimate set by the doctor upon counter-irritation, and especially upon blisters as the best form of it, entirely accords with my own experience. But to ladies the idea of a blister is repulsive. Blisters, like spinal irritation, have gone out of fashion. So, not to injure the *morale* of my patient, I avoid the obnoxious word, and speak of the medical treatment of the system by applications to the spine. This, with me, consists in the rubbing on of the cantharidal collodion, strengthened and rendered anodyne, if necessary, by the addition of a drachm of cantharides and a couple of grains of morphine to the ounce. I tell her that this may occasion some little elevation of the cuticle, with a watery fluid beneath it, which she can let out and all will go on well. No more does it answer to speak to ladies of "spinal irritation;" for the imagination of a sick woman is marvellous past all comprehension, and the words would be a spell of power to conjure up visions of Pott's disease, paraplegia, and every other horror.

As to tonics, I find that commonly these patients do not well bear quinine and iron; that these drugs are likely to cause cerebral congestion and headache. Men of my age are very apt to ride hobbies, and I confess that there is one tonic which I am wont to use almost exclusively in such cases, and which I think free from these objections. That is the sulphate of bebeerine, of which an ordinary young lady can take from three to five grains thrice a day, the latter being my average dose. Alcohol I do not use in this affection. If given in the earlier stages, the patients complain that it makes them dull and sleepy, that their arms hang like lead, and that, instead of supporting, it only weakens and depresses them.

Dr. Austin Flint, Sen.—None but those whose reminiscences extend back twenty-five or thirty years can appreciate the service rendered to practical medicine by the first writers upon this subject. For up to that time the notion prevailed that nearly every disturbance of the internal organs was the effect of inflammation; all the affections now known as functional were regarded and treated as inflammatory. These writers, by directing the attention of physicians to the spinal cord, and to the employment of tonic and invigorating treatment, did much to change the practice of the day.

At that time, under the name "spinal irritation," or, as I chose to call it, "spinal affection," as involving no hypothesis concerning its nature, there seemed to fall a group of cases marked by certain features common to all—tenderness of the spinal column (exceedingly common, but not universally present), susceptibility to cold, coldness of extremities, indisposition to physical or mental exertion, and undue fatigue after either, mental depression sometimes amounting to melancholia. These symptoms belonged to all the cases, and in addition there were those connected with the various organs. But the name spinal irritation fell more and more into disuse ; and, although, for a considerable time, I spoke of spinal affection in my lectures, I at least ceased to do so. Why is this? I recognize to-day the same group of cases which I then recognized. But I have been, for some time past, gradually coming to regard these cases as cases of general anæmia, considering the cord to be in a morbid condition, but a condition due to the altered state of the blood. I have been the more confirmed in this view from knowing that profound anæmia may exist, and yet present none of its superficial signs : an anæmic patient, for example, may have a beautiful color in the face. (I think, however, that anæmia, where present, may always be detected by the venous hum.) But, although I have supposed the anæmia of the cord to be dependent, not upon a deficient supply of blood to that organ, but upon the impoverished character of the blood, yet on this point I should regard Dr. Hammond's opinion as superior to my own.

The adoption of these views has led to a change in my practice. Twenty or thirty years ago I resorted to counter-irritation, especially by means of tartar-emetic ointment. I have now come to look back on those days with a feeling of regret that so much needless suffering should have been inflicted. Yet possibly we may be going too far in the opposite direction. Counter-irritation, by dry cups and other of the less severe means, may be useful. But, looking upon the essential condition as one of general anæmia, I have found my treatment addressed to this very successful in removing the symptoms of the spinal affection. I have no doubt, however, that the addition of electricity, as proposed by Dr. Hammond, would be of great service. One word as to the chalybeates. I think these remedies often have their value underrated from failure to persist in their use. I think that less depends upon the amount of iron given than upon the length of time for which it is continued ; and that, if this be borne in mind, and the chalybeate remedies thoroughly tried, they will be found to give the greatest satisfaction.

Dr. Rockwell related a case of general irritation of the cord, in a lady of thirty years, who suffered from cough, palpitation, dyspnœa, pain in the upper and lower extremities, and in the latter diminished temperature and partial paralysis. Concerning the treatment of the affection by electricity, while recognizing the value of the galvanic current, especially when applied to the sympathetic, the speaker desired to bear witness to the benefit derivable from the faradaic current alone, the negative electrode being placed at the feet, and the positive being passed along the spine.

Dr. Messenger had found muriate of ammonia serviceable in two cases.

Dr. Hammond had no doubt of its value, which a recent article in the *Practitioner* had clearly shown.

In reply to Dr. Chadsey, he said that the cases in which the doctor had found hyperæmia of the cord, upon *post-mortem* examination, were undoubtedly not cases of spinal irritation. This was scarcely ever fatal, while nine-tenths of the cases of spinal congestion ended in death. If we were to judge only by the pain, it might be difficult to distinguish the one from the other; but the history of the case, and certainly a fatal termination, would settle the question.

The galvanic current was a weapon that would cut both ways, and must be used discriminatingly, if we would do good with it and not harm.

With reference to the predisposing influence of sex, his own practice was, like Dr. Barker's, more among women than men, for they were far more subject to nervous diseases in general. He thought they were more liable than men to anæmia; but the opposite condition, congestion, appeared to be more frequent in men than in women, probably because their pursuits led to greater exposure.

The diagnosis from hysteria he had disposed of in the paper, by expressly excluding all cases which did not present the spinal tenderness. If this tenderness were present, the case, though it might exhibit the phenomena of hysteria, was primarily one of spinal irritation, and to be treated as such.

With regard to stimulants, his own experience was decidedly in favor of alcohol, in its stronger forms, as the best of all remedies for this affection; and the patients would bear it in large amounts. A weak girl would drink half a tumbler of brandy-and-water, and feel it no more than a strong man. He had found that these patients did not bear iron well at first, but he had employed sulphate of manganese, in doses of five and ten grains, with good success.

The President was glad to hear Dr. Flint's remarks in deprecation of the more violent counter-irritants. He remembered the case of a lady of the greatest pluck and endurance, long a sufferer from spinal irritation. Other remedies failing, he had finally proposed a camphor moxa, which she had gladly accepted; but it did no good. There lay also upon his conscience the application of nitric-acid issues over the spine in two or three cases, with no better result. He had since abandoned these powerful irritants, leaving their indelible scars, and confined himself to the tincture of iodine, applied as often as requisite.

In searching for the cause of this disease, we had before us the patent fact that it is most frequently seen in young women, between the ages of fifteen and twenty-five. This was the most active period of woman's ovarian life; and the time must probably come when the relation between these facts would be better understood.

Dr. Bulkley added his testimony to the value of tincture of iodine.

Dr. Peaslee had found those cases of dorsal spinal irritation, attended with dyspeptic symptoms, almost invariably accompanied by general anæmia. Such cases were not uncommon in females. There was one preparation of iron which commonly agreed well with them, if the liver were kept active, say, by rhubarb and hyd. cum creta. This was the iron by hydrogen, which might be given in doses of two grains, combined with a grain or a grain and a half of extract of hyoscyamus. After trying this for a time, the extract of gentian might be substituted for the hyoscyamus; and, after that, some other preparation of iron would commonly be well borne. In connection with this treatment he used blisters where the patient was not extremely anæmic, or the reverse of these, belladonna-plasters, where the anæmia was excessive. Twenty-five years ago he had seen much of the treatment by tartar-emetic ointment, and its results were so bad that he had scarcely ever employed it. He had seen patients scarred over the whole length of the spine with this ointment, who turned out to have only uterine disease or displacement, till he was almost forced to the conclusion that spinal irritation was a myth, and that the term should be discarded. But other cases, occurring in connection with uterine disease, had soon convinced him that they were better characterized by this name than by any other. The diagnostic mark was that pointed out by Dr. Hammond—tenderness on pressure over the spine.

The Society adjourned.

Miscellaneous and Scientific Notes.

[The great accumulation of matter on hand and in type compels us again, and very reluctantly, to omit all Bibliographical Notices and Reports on the Progress of Medicine from the present number of the JOURNAL.]

ANÆSTHETIC INHALATION.—Through the courtesy of Prof. George T. Elliot, M. D., of this city, we have received for publication the following letter from Sir James Y. Simpson. This letter is in reply to some strictures made by Prof. Jacob Bigelow, M. D., of Boston, on Prof. Simpson's speech delivered in Edinburgh in October last, on the occasion of the presentation to him of the freedom of the city. The letter is a copy of the original addressed to Dr. Bigelow, and was forwarded to Dr. Elliot with the request that it be published in some medical journal of this country:

EDINBURGH, *January* 3, 1870.

DEAR SIR: There has been sent to me, from America, a Chicago newspaper containing a letter of yours which is alleged to have been published in a late number of the *Boston Medical and Surgical Journal*.[1] In this letter you speak of the bestowal upon me some months ago by my fellow-towns-men of the rank of an honorary burgess of Edinburgh, and comment in terms of bitterness upon the subject, and upon what I said, or rather upon what I did not say, on that occasion.

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[1] This letter appeared in the editorial columns of the journal mentioned under date of November 25, 1869.—[ED. N. Y. MED. JOUR.

I feel assured that, if you or any one else had felt as nervous and timid as I did on rising to address the public meeting which witnessed the presentation, you would not be astonished at any thing I did allude to or did not allude to; or that I failed in adverting to numerous matters to which I might have adverted.

The gravamen of your charge is this: In his extempore address to me on the occasion in question, the lord provost thought fit to allude to some of my professional investigations, and especially to those bearing on anæsthetics, acupressure, and hospitalism. He spoke of the application of *chloroform* to the assuagement of human suffering as among the "greatest of medical discoveries in modern times." In replying on the spur of the moment to these remarks, I stated simply in a sentence the extent to which chloroform was now used for anæsthetic purposes, by adverting to the great amount of it manufactured by one single firm at the present day. I might, if there had been time, have added evidence of the extent to which it has superseded all previous anæsthetics, by stating the amount of its manufacture by other firms here and elsewhere. But I had many other subjects to advert to besides chloroform, and only a few short minutes within which I was expected to include them all. According, however, to your views, I am very deeply blamable for not taking up a subject which the lord provost did not allude to, viz., the history of anæsthesia. You hold that I should have entered to a greater or less extent into some historical notice of anæsthetic agents. The history of them has always taken me a full hour in my university lectures, and in those lectures I have year after year heartily paid every due compliment to the most important part borne in the consummation of the practical application of anæsthetics by America, particularly by the cities of Hartford and Boston, and especially by the energy and genius of Dr. Morton. Surely, however, it would have been sadly out of place on such an occasion and with such an audience to have shown that, before I discovered the application of chloroform to anæsthetic purposes, numerous other agents had been previously suggested and used for the same object—as sulphuric ether by Drs. Jackson, Morton, and Marcy—as carbonic acid by Dr. Hickman, in imitation of the experiments performed for ages on the poor dogs at the Grotto del Cani, and as nitrous oxide (an agent extensively employed as a dentist's anæsthetic at the present hour), and which was first proposed some seventy years ago for "destroying physical pains during surgical operations" by Sir Humphrey Davy; or should I, in your opinion, have even gone still farther back in therapeutic history, and described what, doubtless, as a former lecturer, you are well acquainted with, namely, the other soporific vapors and measures employed by different older surgeons in Greek, Roman, and mediæval times, with the view of rendering their operations painless to the patient? In that way I might have easily shown that the idea of making a patient anæsthetic, before subjecting his body to the knife or cautery, was a kind of knowledge familiar even to non-professional writers of mediæval and of later times, and that some theological authors, like Origen, for example, in the third century, allude to the artificial production of anæsthesia in surgery as a well-known practice; while, in reference to Scotland, I might have cited Abbot Bower, who lived and wrote about the year 1400, within ten miles of Edinburgh, as telling us by what means anæsthetic surgery was accustomed to be effected in those days, and what they gave to patients "*secandi ut possent sine dolore secari.*" Or I might have adduced the Monk Joceline as alluding with circumstantial details to an alleged instance of it in the hygeology of Scotland as early as the sixth century. All this, and much more, might have been mentioned; but all this would have been, in my opinion, though not apparently, in your opinion, totally misplaced and grievously out of order, as much so as a disquisition on the previous means of arresting surgical hæmorrhage in wounds by ligatures, torsion, etc., would have been when I adverted for a moment to the subject of acupressure. In the way of a climax, you terminate one of the paragraphs in your letter with the statement that I was not the "first man" to inhale a vapor to such an extent as to destroy sensibility. Most certainly I was not, and certainly I never was so intensely foolish as to claim to be so. In the course of my investigations I have, however, experimented upon myself with various vapors, the innocuous or the poisonous effects of which upon the economy were previously altogether unknown and unascertained, and I have sometimes suffered in consequence. As a professor of therapeutics, you must surely be well aware that the first experiment of breathing a vapor to such an extent as to destroy sensibility was made neither in America nor in our own days. Without adverting to the acknowledged fact that it was accomplished with the vapors driven off from hypnotic vegetable extracts by other surgeons, from Hugo de Lucca and Theodoric downward, let me remind you that Sir Humphrey Davy boldly (and notwithstanding that he had witnessed occasional deaths on animals from it) made the experiment to which you advert many times upon himself in the last year of the last century with nitrous oxide, and found that headache and other pains disappeared under its influence.

About forty years ago Faraday, in this country, and Godman in America, showed, as the result of their observations and experience, that the effects of the inhalation of the vapor of sulphuric ether were quite similar on the nervous system to those produced by the inhalation of the vapor of nitrous oxide gas—a truth subsequently proved by many pupils in many chemical and other schools in your country, as well as in mine, by their inhalation of ether. Your remarks, so far as I understand them, imply that it is your belief that Dr. Morton was the "first man" of "sufficient courage" to breathe "a vapor" so as to produce a state of anæsthesia. But you must know as well as I do, from the official documents laid before the Senate of the United States, that this is doubtful as regards the course of matters even in America. For it appears on these documents—1. That Dr. Jackson avers that he breathed for this object sulphuric ether earlier than Dr. Morton. 2. That before Dr. Morton made the same experiment upon himself, in 1846, he made it first upon others, and particularly upon his pupil, Mr. Speirs; and 3. That two years previously, or in 1844, Dr. Marcy, of Hartford, had successfully excised a tumor from a man who had been rendered anæsthetic for the purpose, by the vapor of sulphuric ether, while at that same early date, in the same city, Dr. Horace Wells had extracted teeth from a dozen or more patients rendered insensible by inhaling nitrous oxide according to Davy's suggestion.

There has lately been raised, I am told, in the city of Boston, a monument,[1] in commemoration of the employment of anæsthesia in surgery in that city in 1846. But have the erectors of this monument cut upon it the names of either of your fellow-citizens, Dr. Morton or Dr. Jackson, as the first investigators, or the names of Warren and Hayward, as the first Boston hospital surgeons who operated upon patients under the influence of sulphuric ether? Or have they generously inscribed upon its sides any allusion to the fact that two years previously anæsthetics had been inhaled successfully in dentistry and surgery in the neighboring city of Hartford? I have been assured—though it is scarcely credible—that there does not appear upon the monument the name of a single American chemist, dentist, or surgeon. Why is it so? You have the monument. Have you not had the men?

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[1] A full description of this monument will be found in this JOURNAL, August, 1868, vol. vii., p. 463.—[ED. N. Y. MED. JOUR.

You commence the concluding paragraph of your letter by averring that anæsthetic inhalation "began" (to use your own words) "in this country" (America), "and was first used in the extraction of teeth, and afterward in capital operations, in the Massachusetts General Hospital, and in obstetrical practice." Your words so far affirm that anæsthetic inhalation, besides being first employed in America in dentistry and surgery, was, in your country, also "first used" in "obstetrical practice." You must excuse my saying that this last observation is unaccountably incorrect. The use of anæsthetic inhalations in obstetrical practice was begun and extensively followed out in Edinburgh weeks, or even months, before it was tried in Boston, or in America. The first case of midwifery in which sulphuric ether was adopted as an anæsthetic occurred here under my care on the 19th January, 1847. On the 1st March, 1847, was published by me, in the *Edinburgh Medical Journal*, an essay on the subject, containing a series of obstetrical cases, and a lengthy discussion of the question of the applicability of anæsthetics to midwifery. It was not, however, according to the published evidence of your townsman, Dr. Channing, till April 7th, that the first case of the employment of anæsthetics in midwifery occurred in America, and the second did not take place till 5th May. (See Dr. Channing's treatise on "Etherization in Childbirth," p. 26.) But, before the date of these two cases, the practice had been fully established in Edinburgh and elsewhere. Perhaps you and I, as parties implicated, are not adequate judges as to whether your statement on this point is candid and credible, or utterly the reverse, but I willingly leave the decision of this to the feelings and verdict of an honorable profession. You think that I am greatly blamable because, in the way of omission, I did not advert to the previous application of sulphuric ether in America as an anæsthetic, when the employment of chloroform was referred to. I think, on the contrary, that you are infinitely more blamable than I am, because, without the slightest reason or ground, and in the way, not of omission, but of deliberate commission, you have, in this letter of yours, attempted to appropriate for your city and country what indubitably belongs to my city and country—namely, the credit of the first introduction and establishment of anæsthetic inhalation in obstetrical practice.

I have the honor to be, yours truly,

J. Y. Simpson.

To Dr. Jacob Bigelow, Boston.

Death of Dr. J. W. Southack.—At a meeting of the New York Pathological Society, on Wednesday evening, January 12, 1870, the following preamble and resolutions were adopted:

Whereas, in the death of Dr. J. W. Southack, this Society feels that it has lost a member whose character, energy, talents, and requirements, had already proven that, had life been spared, he would have attained an eminent position in his profession, and in that branch of it which this Society investigates:

Resolved, That while we recognize Providential ordering in this sad event, yet we mourn his loss as that of one who seems scarcely able to be replaced—as one which this Society has special reason to regret.

Resolved, That we tender to his family, in this bereavement, our sincere sympathy.

Resolved, That these resolutions be published in the medical journals of this city, and that a copy of them be presented to the family.

Lewis A. Sayre, *President*.

George F. Shrady, *Secretary*.

The Medical Society of the District of Columbia has made an appeal to Congress, in reply to the resolution which was introduced in the Senate by the champion of the colored men, Mr. Sumner, with the view of repealing the charter of the Society. This reply was prepared by a committee, consisting of Drs. Lovejoy, Liebermann, and Toner, in connection with the President, Dr. W. P. Johnston. The charter of the Society secures to all medical practitioners, without distinction, rights dependent only upon certain moral and intellectual qualifications; and the Society, as we have before stated, did not in any way desire or attempt to interfere with or abrogate these rights in the case of Drs. Purvis and Augusta, the colored physicians. The same instrument leaves the question of membership one of optional and social association, and this right the Society exercised, and declined to accept Drs. Purvis and Augusta as their companions in their social meetings, as they had an unquestioned right to do.

Dr. March's Case.—We have received, from Albany, a reprint copy,[1] from the present number of the Journal, of Dr. McNaughton's reply to Dr. Robertson's criticism on the original paper giving the account of the case. This reprint has pasted in it a slip which does not appear in the JOURNAL communication, and is worded as follows:[1]

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[1] This reprint was issued in advance of the Journal, to allow of its distribution at the meeting of the State Medical Society.

> Dr. Armsby never, to my knowledge, desired or expressed a wish to buy or suppress a negative of the picture of Dr. Marsh's *(sic)* disease. He always purchased an equal number of copies of both, to send or give away.
>
> E. S. M. HAINES,
> Photograph Rooms, 478 Broadway.
>
> ALBANY, *January* 29, 1870.

As pertinent to this matter, and as a part of the history of the controversy which has risen upon this case, we publish another statement by Mr. Haines, which has also been forwarded to us from Albany:

> On the 18th of June, the day after the death of Dr. March, Dr. Henry R. Haskins, Demonstrator of Anatomy in the Albany Medical School, brought to me a bladder and prostate gland, said to be from the body of Dr. March, and desired a photograph. Dr. Haskins called afterward and stated that Dr. Armsby, who was out of town when the photograph was ordered, was not pleased, for the appearances did not present themselves to suit him, and wished no more copies of this photograph to be sold. A few days after, he confidentially asked me what I would take for the negative. I asked him whom he wanted it for, but he would not tell me; said no matter, and he wished no more to be said about it. I presumed that Dr. Armsby wanted it, but I refused to sell it.
>
> On the 25th day of July the same specimen, which had been preserved in some fluid, was brought to me again, for the purpose of having me take another photograph. After the specimen had been gotten ready (in a manner so unusual as to occasion my subsequent comments), by Dr. Armsby, having moulded it, as I have said, like *putty*, and trimmed it to his satisfaction, I took the negative picture. Copies of this negative only Dr. Armsby wished to be sold, as Dr. Haskins stated.
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> I did state, to both Dr. Edward R. Hun and to Dr. Robertson, what I believed to be the fact, that Dr. Armsby was chiefly interested in this matter, and that I regarded Dr. Haskins as his agent. Since Dr. Armsby did not personally express to me a wish to buy or suppress a negative of the picture of Dr. March's disease, I did not refuse to sign the paper, which he brought to me, for it was technically true; but I did not sign it with any intention of contradicting Dr. Robertson.
>
> E. S. M. HAINES.
>
> Sworn to before me, this 7th day of February, 1870.
> J. M. BAILEY, *Notary Public.*

We may state here that, so far as this journal is concerned, we consider the controversy should now end. Both parties have now had an opportunity to bring forward their sides of the question, and we do not deem that the interests of science or a regard for the truth call for any further comment. The opinion of our readers, and of the profession at large, is undoubtedly well formed on the merits of the case, and probably no further discussion would materially alter such opinion. We deem it proper, however, to add that we have received from Dr. Robertson a rejoinder to Prof. McNaughton's reply. This rejoinder is most courteous in language and tone to Prof. McNaughton, and expresses the greatest satisfaction at the appended letter of Prof. Gross, which he (Dr. R.) claims fully substantiates the view he took as to the proper course to be followed in the treatment of the case.

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[1] Since this paragraph was written, and in type, we have received from Prof. Lansing, of Albany, presumably by Dr. McMaughton's advice and consent, a request to insert in the JOURNAL this slip. This request we have acceded to, and the above comment, therefore, is no longer strictly true. As a simple matter of justice, however, we deem it advisable to let the paragraph stand as written, and to give both sides of this singular transaction.—[ED. N. Y. MEDICAL JOURNAL.

REPLY TO DR. C. A. ROBERTSON'S REVIEW OF THE REPORT CONCERNING THE LAST ILLNESS OF DR. ALDEN MARCH.

E. S. DUNSTER, M. D., *Editor New York Medical Journal:*

SIR: A pamphlet, purporting to be a reprint of an article from the NEW YORK MEDICAL JOURNAL for January, 1870, is in circulation in this city. The title-page is as follows: "'Lo! the Cranes of Ibycus.' A Review of the Report concerning the Last Illness of Dr. Alden March, with Critical Comments on the Improper Medical Treatment of the Case and Strictures on Pitiful Devices for Concealment. By Charles A. Robertson, A. M., M. D., etc."

The pamphlet has been circulating in this city since the 20th of December, 1869, and we hear, since, that the same has been scattered broadcast through this State, and perhaps beyond it. The circulation of the NEW YORK MEDICAL JOURNAL will carry the criticism to Europe, as a specimen of medical reviewing in the New World. In our own country it will, of course, attract attention, on account of its connection with Dr. March, so well known to the profession as an ex-President of the American Medical Association.

The title-page is well calculated to attract attention in this locality, where some of the parties referred to have been long known, and where others, believed to be connected with the criticism, are in a fair way of becoming better known than heretofore, and who will in future doubtless be duly appreciated.

It is not my purpose to notice more than a few of the errors and misrepresentations of the review. The readers of the JOURNAL have the report of the case reviewed in their possession, and can refer to it, and compare the one with the other. Then I would request them, before they come to a decision, to consider the report and the criticism in connection with the remarks contained in this paper. Having done so, every fair-minded, respectable member of the medical profession in this State, or elsewhere, into whose hands the pamphlet or JOURNAL may fall, can decide whether the criticism of Dr. Robertson was dictated by zeal for the promotion of truth, the correction of error, and the good of the medical profession; or rather by inordinate vanity and self-conceit, or a desire for notoriety, or from some baser motive. Whatever the governing motive was, few medical men of respectable standing will deny that the spirit and language of the review are in flagrant violation of the recognized code of medical ethics.

The reviewer commences by giving so much of the report of the case reviewed as suited his purpose, and distorting what did not. He manifestly started with the determination of finding something wrong in the management of the case, and what he could not find he imagined, or saw in the report, through his mental ophthalmoscope.

It is stated, in the report of the case, that the patient, after fatigue and exposure, had heat, restlessness, thirst, and other febrile symptoms; but that his most troublesome symptom was an irresistible desire to void urine every fifteen or twenty minutes. It is also stated that early attention was drawn to a tumor, occupying the lower part of the abdomen, and distinctly traceable from the pubes to the umbilicus, but much better defined on the left side of the mesial line than on the right. On the right side of that line, the tumor was not so well defined, or traceable to the pubes; but yielded a flat or dull sound on percussion. The tumor was regarded as a distended and thickened bladder, bound on the left side by adhesions to the omentum and abdominal parietes. The critic is not satisfied because the reporters did not say "exactly what the diagnosis was," and that the reader is left to draw his own inferences. Dr. Robertson has done so with a vengeance! He has not only gone outside the record and *made* a case to suit his own purpose, building it upon *post-mortem* recollections that are contradicted, upon breakfast conversations that are denied, upon whispers and rumors and what he terms "inevitable" implications; but also, without seeing or knowing any thing personally of the symptoms or indications, or progress of the case, he has criticised the treatment of those in charge, and given his own views of what should have been done, incidentally showing his great knowledge of pathology, physiology, and therapeutics, for the benefit of those who drew up the report.

That was very obliging in him, and should be duly acknowledged.

The reviewer calls especial attention to the recollections of what was seen by Dr. Edward R. Hun, at the *post-mortem* examination, and makes them the basis of much of his criticism. He says they were prepared at the request of Dr. Armsby, and received his approbation. Dr. Armsby thanked Dr. Hun for furnishing them, but never especially approved them. Besides Dr. Hun, four other medical gentlemen, at the request of Dr. Armsby, furnished their recollections of what they observed. A copy of one of these reports, attested by the gentlemen whose names are appended to it, will be found at the conclusion of this paper.

A copy of a letter to me from the eminent Dr. Gross, whose standard works on surgery, pathological anatomy, and the diseases of the urinary organs, are so well known to the profession, is also, with his permission, hereto appended. How conclusive the examination and description of the bladder and kidneys of Dr. March, contained in this letter, are, in reference to facts stated in Dr. Hun's report, and upon which the reviewer so confidently builds his criticism, is left for the profession to decide.

Dr. Hun says, "There was no abnormal thickening of the walls of the organ." The reverse of this is true, according to my own observation, the report signed by Dr. Stevens and others, and according to the measurements of Dr. Gross.

In Dr. Hun's report it is stated: "It was also observed that the connective tissue, lying anterior to the prostate gland and neck of the bladder, was stained and infiltrated with blood, although there was no evidence of any urinary infiltration."

None of the other gentlemen, preparing reports of the autopsy, say any thing about stain or infiltration of blood in the part referred to. Now, I was at the time better situated than most of these observers for seeing the dissection, and saw the blood referred to, for my attention was called to it by one who stood near to me. The blood would not be thought of, if one had not a purpose to serve in looking for it. It was not infiltrated, and was easily wiped off by the finger of one of the dissectors. There was no neck to the bladder, in the ordinary sense of the term, but the urethra passed through the enlarged prostate for two and a half inches or more, when it reached the transverse ridge, at the bottom of which the *cul-de-sac* referred to by Dr. Hun was situated, and arrested the further progress of the catheter in that direction. The fore part of the prostate touched the pelvic face of the pubic ligament, and in detaching the bladder from its connections to the pubis, and in cutting through the membranous part of the urethra, as it passes under the pubic arch, blood, of course, would come from the congested veins, to make "a stain or infiltration of blood," but would not give any evidence of infiltration of urine, because all the urine had been drawn off before the urethra was cut across. There was no evidence that the parts about the membranous urethra had been bruised or injured at any time by attempts to introduce a catheter. The same remark may be made in regard to the prostate outside and inside. It lay behind the symphysis pubis, as large as a goose-egg, firm, and without a bruise or scratch.

Neither do any of the other gentlemen, making reports, insinuate what is implied when Dr. Robertson quotes Dr. Hun as saying "that the pelves of both kidneys were distended, as is usual in cases of retention," or what is implied when Dr. Hun himself says, in his report: "The pelves of both kidneys were enlarged, but, whatever fluid they may have contained, escaped unnoticed when the ureters were divided."

As to the relative size and healthy condition of the kidneys, there is a direct variance both as to fact and opinion between what is stated in Dr. Hun's report of the autopsy, and the other reports, and in the letter from Dr. Gross.

Indeed, the little slip inserted in the pamphlet, cautioning against applying to the report of the autopsy, by Dr. E. R. Hun, the expressions used in the text, of "many errors of judgment," and "many mistakes," would seem to have been really necessary.

The critic continues: "The case was professionally an interesting one, and Dr. March was personally a man held in high consideration." Did Dr. Robertson so hold him? "It is well known that, in this locality, great curiosity was felt to learn all the features of his illness, and it is no secret that, after his death, the animadversions respecting his treatment were *censorious* and *severe*." I believe the last sentence to be true, and that chief among the curious, *censorious*, and *severe*, was Charles A. Robertson, the *Oculist.*

The critic takes particular pains to give the differential diagnosis between *cystitis* and retention of urine, and comes to the conclusion that the case under review was one of *simple retention*, and that the proper treatment would have been to have at once introduced a catheter. That there was *detention* of urine, was believed to exist by the patient, as well as by his medical advisers, and the employment of the catheter was proposed, but was resisted by the patient, himself an eminent surgeon, and in full possession of his mental faculties. He was passing from two to three quarts of urine in twenty-four hours, and the tumor was no larger at the end of a week, after the last attack began, than it was when he was first confined to the house.

Several days after his confinement, the patient suffered great distress from inability to void urine, at a time when Dr. Armsby and myself were absent from the city. Then Dr. James P. Boyd, together with the patient's son, a graduate of some fifteen years' standing, advised the introduction of a catheter, to which the patient, *for the first time*, gave his consent. The attempt was made, but did not succeed in drawing off the urine.

The report says: "The instrument passed without difficulty its whole length, without entering the bladder; blood coagulated in the catheter, and no *urine* passed through it, but some (urine) passed external to it (the catheter) and followed its withdrawal." The reviewer quotes this sentence in his review as follows: "The instrument passed without difficulty its whole length, without entering the bladder, but bringing away clotted blood."

He also quotes it in a printed letter, addressed to the medical students of the Albany University (which printed letter is part of the *res gestæ* of the review), as follows: "Note carefully what is said about passing the catheter; that blood coagulated in it, that *some* (not urine, but blood) passed external to it and followed its withdrawal." From these garbled versions of the first attempt to use the catheter, and the "stain of blood infiltrated in the connective tissue around the membranous urethra without any evidence of urinary infiltration," he charitably asks the professional reader to believe that "a false passage had been made during life by thrusting the point of the catheter through the anterior wall of the membranous urethra," and these students to say "whether somebody had not been *ramming* this instrument into the substance of Dr. March's body, instead of along the natural channel." Oh, shame, where is thy blush? He further adds: "If Dr. Armsby did not do this, perhaps he will say who did, for we are eager to know." The only time Prof. Armsby attempted to introduce the catheter was a few days before the patient's death, and I can vouch for it that the attempt, although not successful, was made in the most gentle and careful manner; and that the *post-mortem* examination has shown no evidence of a false passage made by any body, and that a false passage had no existence except in the brains of those who wished to find one.

Within twenty-four hours of the time of writing these remarks, the bladder and its appendages were carefully examined by Dr. Armsby and myself, in presence of Dr. Francis Burdick, of Johnstown, N. Y., one of the gentlemen who signed the paper concurring in the correctness of Dr. Hun's account of the dissection, and referred to in the review as a "prominent surgeon from a distance." No trace of a false passage could be discovered, and Dr. Burdick said, distinctly, he had not seen any evidence of a false passage, and did not think that such a passage was implied by any thing he witnessed in the examination, or in the report of the autopsy given by Dr. E. R. Hun.

The description in the review of the sufferings of a person laboring under *real retention* is good, but not applicable to the case under consideration. There was not at any time complete retention; for the patient never passed less than two or three quarts in twenty-four hours, until the last two days of his life. But, as he had before his illness been in the habit of voiding from four to six quarts in a day, it is asked what had become of the difference (between four and six quarts and two or three), when the patient continued to drink very freely of iced water and diluents. It is easier to ask such a question than to answer it correctly. It is difficult to say how much of the water drank passed off through the skin, the lungs, or the bowels, but it is certain that the difference (one to three quarts daily) did not accumulate in the bladder. It is asked, if any considerable portion was collecting there, was not this an appalling consideration? It undoubtedly would be; but such was not the case at any time. There was probably more urine in the bladder at the time the patient died, than at any time during the last illness, because less was voided the last two days than before. There was, during the last attack, distention of the bladder habitually, but no *over-distention*, as that term is usually understood. Owing to adhesions on the left side of the bladder, and the "enormous" enlargement of the prostate, and the deep pouch behind the prostate, it is probable that for months Dr. March's bladder never contained less than a pint of urine; for, from the state of the prostate and the adhesions of the bladder to surrounding parts, it was physically impossible to empty it by voluntary efforts, and the bladder itself could aid but little.

The case of the old Frenchman, mentioned in "Watson's Practice" (page 744), if quoted as originally intended, to enforce a caution against mistaking a distended bladder for dropsy of the abdomen, would be well, but to torture its lesson, and to compel the indefinite number of "quarts of turbid, stinking urine," drawn from his "enormously distended" bladder, to serve as a parallel illustration, of what "might have been" in Dr. March's case, smells of something stronger than fair and honest criticism.

We are told that there was no danger of uræmic poisoning from retention of urine in the bladder—that it "never occurs directly from urine retained in the bladder." If that be so (which admits of doubt), one source of danger is removed from those so afflicted.

In the report of the case it was mentioned that the *urea* in a given quantity was less than the average, but that, as a larger quantity of urine than is common was daily voided, it was hoped that enough of that excrementitious constituent was secreted to prevent uræmic poisoning; that it was not probable uræmia was often caused by absorption from the bladder, but that, if uræmia took place in connection with retention of urine, the fault was in the kidneys not secreting what ought to be thrown off, as happens in ischuria renalis, when neither urea nor any urine at all is secreted, but retained in the blood.

The reviewer tries to mystify this statement, which seems plain enough to be understood.

The reviewer thinks the kidneys were healthy; that the fact that they "secreted abundantly, and that no morbid product whatever existed in the urine is conclusive that any pathological change discovered in the kidneys must have been exceedingly trivial, and had nothing whatever to do with the death of the patient."

I think the kidneys were *not* healthy. The same opinion is held by other truthful and competent observers. I did not infer *both* kidneys healthy from a microscopical examination of *one* of them, and that from a minute section of perhaps its sound portion, but I inferred that both kidneys were not healthy from their general appearance, and the visible traces of disease exhibited in their structure.

But aside from these—the fact of congested renal tissue—the existence of several cysts in their cortical substance, their excessive secretion of from three to six quarts of urine daily, for several months before—is conclusive, to my mind, that the pathological changes in these organs were far from being "exceedingly trivial," and that they did have something to do with the death of the patient.

Finding that the catheter could not be introduced so as to draw off the urine, what does the reviewer propose to do next to give relief in the case? Those in charge did not think that the bladder was dangerously *over-distended*, the fever had subsided, and hopes were entertained that the patient would yet gradually get better, though not well.

The reviewer is a bolder man. He gives a graphic description of a ship at sea—"that same old ship," whose stately sailing through our inland sea is so familiar to us here—and compares the patient with retention to such a ship in a storm off a lee shore, the pilot seeing the dreaded land "through a rift in the clouds," etc. After a fine display of nautical manœuvring, it all ends in advising "puncturing the bladder," through the rectum or above the pubis, and if that was not advisable, then to have recourse to inter-pubic puncture as less likely to be followed by urinary infiltration. If the bladder was not dangerously over-distended, if there was no hazard of uræmic poisoning by absorption from the bladder, and if the patient until the last twenty-four hours of his life was passing daily two or three quarts of urine, what good would any puncture of the bladder do beyond very temporary relief? It could not restore the prostate gland to a healthy condition, or relieve the bladder from its adhesions, or renew the tissues of the kidneys; and, if the operation should seem to hasten the patient's death, would not the same persons who saw or imagined infiltrated blood, and a false passage "*without urinary infiltration*," see infiltration of both *blood* and *urine*, and more too, to account for the patient's death, and to blame the medical advisers for bad treatment?

Much is said in the review about different photographs having been taken of the morbid specimens; of manipulations, and of torturing by means of hooks, and pins, and guys, a simple "pouch of the bladder" "into something quite deep and abnormal in appearance."

The ingenuity of the reviewer is "severely taxed" to explain, in some malicious way, the fact of Dr. Armsby having had two sets of photographs taken of the bladder of Dr. March,[1] the one when it was in a fresh condition, and the other after it had been immersed in alcohol. I am sure there was no concealment about the transaction. There could be none. Copies of both sets were distributed at the time to eminent surgeons in different parts of the country. The second set was thought to represent the remarkable peculiarities of the specimen better than the first.

There were no fabrications of mucous membranes nor any cunning manipulations to photograph a lie. That were impossible. Upon examination of the preparation now before me, and which is accessible to any who may desire to examine it, I find every thing as natural and as little changed as could be expected in any preserving fluid. That must be a "desperately wicked" ingenuity that would weaken faith in all photographic representation, and throw discredit upon the correctness and value of all pathological specimens in the great medical museums of the world, in order that this single specimen may not be permitted to show itself exactly as it was and is.

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[1] Dr. Armsby never to my knowledge, desired or expressed a wish to buy or suppress a negative of the picture of Dr. March's disease. He always purchased an equal number of copies of both, to send or give away.
E. S. M. Haines, Photograph Rooms, 478 Broadway.
Albany, *January* 29, 1870.

The reviewer undertakes to speak for Dr. Gross, and to tell us what he means in his "Pathological Anatomy" by "transverse ridges of mucous membrane." Dr. Gross, in his letter to me, speaks for himself both on this and other points connected with this very specimen.

As to the facility of passing an ordinary catheter after death, and difficulty before, a few words will suffice. The catheter passed under the arch of the pubis without difficulty; but, the moment it entered the prostate, it was surrounded by the dense and enlarged prostate, until it reached the cross-bar and *cul-de-sac* referred to in the report of the autopsy. After death the abdomen and parietes were divided, the urine drawn off, the bladder opened in the mesial line through the prostate near to the pubis, leaving undivided not more than an inch of the prostate, where the urethra enters it. When the catheter passed the pubic arch its point passed on the same level until it emerged into the open air, where the solid prostate had been divided down into the canal of the prostatic urethra—a very different matter from introducing it during life.[1]

I have bestowed more attention on this review than it deserves, but it has been spread with such assiduity, and to such an extent, that I deemed some notice proper. It is stated that "two of the signers of this paper are professors in the Albany Medical College;" that the position of *professor* has been degraded by this attempt to give consideration to such fallacious theories and *detestable* methods. It is said of the report of the case that its fabrication is artful, specious, and sophistical, etc.

I am responsible for that part of the report which relates to the treatment and *post-mortem* appearances found, and am not aware that it deserves such epithets. It was prepared to counteract the slanderous reports in circulation about the case, and contains all the information the friends of Dr. March would be likely to wish for, or care, unless for some special reason, to know.

Much offence is said to have been taken at the reference made to some gentlemen of "high character and experience," who "were inclined to believe that the tumor, which was distinctly felt, was not simply a diseased and enlarged bladder, but had connected with it, and external to it, another growth probably of a malignant character. This opinion was not shared by the attending physicians. Nothing in the *post-mortem* examination was found to confirm its correctness."

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[1] Speaking of the anterior surface of the third lobe of the prostate, or that portion looking toward the meatus, Dr. S. O. Vanderpoel, in his account of the *post mortem* says: "It is well known that at this portion there is in health, at the fore part of the veru montanum, a depression forming a *cul-de-sac* about one-fourth of an inch in depth, running upward and backward into the substance of the prostate. Now, the antero-posterior enlargement of this lobe had increased the depth of this *cul-de-sac* to one half an inch, so that the point of a catheter entering would be immovably fixed, while the form of the enlargement would almost inevitably drive the instrument into it."

Persons must be very sensitive who would be offended by being referred to in such terms. The language contrasts favorably with the rude and vulgar terms employed by the reviewer. The attending physicians could not know during life *precisely* what was the nature of what lay external to the bladder, on the left side where the tumor was distinct; but I never heard any one express the opinion that the whole tumor was solid, or that the bladder contained no water.

The reviewer closes with a tirade against the practice in the case, and against the two professors who concurred in it. He further says: "Fortunately there are professors *there*" (in the Albany Medical College) "who scout such practice."

I have not yet found out who those professors are, but if among them there be any who, by sympathy or suggestion as to its preparation, have helped to shape it to its present form and give permission for the use of terms like the above, or who by subsequent approval have encouraged its publication, why then the glory and the shame of the production should be shared by them with Dr. Charles A. Robertson.

The three men who had the principal charge of the case were not inexperienced. They have doubtless had far more to do with such cases than Dr. Robertson, and were much more competent to judge what was proper to be done than those who never saw the case and had no knowledge of the circumstances which preceded or attended the attack. The practice pursued I contend was "usual in such cases."

Soon after the death of Dr. March, I was called in consultation with two experienced surgeons, about a case of *complete retention of urine.* I advised that, before the operation of puncture be resorted to, the bowels should be freely moved by castor-oil—to put the patient in a warm bath, and follow it by a full anodyne injection—then to try the catheter—failing in that, to puncture the bladder. The advice was followed—the remedies had the desired effect—the man passed his urine without resorting to the catheter, and passed it with more ease than he had done for several weeks. This is only one of many cases I have had where similar treatment had the same good effect; and I do not recollect, in my whole private practice of fifty years, of a death from retention of urine.

I do not wish to say any thing unkind of Dr. Robertson, or to injure him in his own specialty as oculist and aurist. He has been only a few years in this city, and, until he engaged in the preparation of the pamphlets referred to. I had been friendly to him, and had aided him in getting patronage in his profession. In this locality it is more than suspected that he has been encouraged in his course, by others, who, like himself, bore no good-will to Dr. March during life, and who bear no good-will to those who signed the report concerning his last illness. The pamphlet referred to has been scattered profusely in this city, not only among the profession, but among merchants, bankers, and others, as well as among the families usually attended by the medical advisers in the case, and some one had the brutality to send it to the family of the deceased.

Had the case been as badly treated as the reviewer would have his readers believe, is it agreeable to medical ethics that he should unnecessarily rush to blaze it before the public? There is not, I venture to say, a decent man in the profession, or out of it, who will not condemn the spirit and manner of the review, from whatever motive it originated; and, if others have aided and encouraged the publication of the slander, they will share in the condemnation they all deserve. If, from the grave of the old surgeon, a voice could be heard, it would be in tones of scorn and reprobation of the hypocritical wailings and unholy uses that his case had been made to serve as a pretext and occasion.

James McNaughton.

Albany, N. Y., *January*, 1870.

Notes on the Autopsy of Dr. March.

Externally there was nothing noticeable, except that the well-rounded limbs and body indicated that there had been no long and exhausting illness, but that he had passed suddenly from a state of perhaps more than usual robust health; and except that, on the left side of the abdomen, there could be seen a rounded elevation, extending from a point as high as the umbilicus to the brim of the pelvis, feeling hard and somewhat elastic, and suggesting a tumor within the cavity of the abdomen.

The abdomen being laid open, the walls were shown to be thick with adipose, and, to all appearance, healthy. There was no indication of disease of any of the viscera except of the bladder and kidneys.

The bladder was greatly enlarged, and extended upward, and to the left as high as the umbilicus. Strong and thick bands of fibres bound it firmly to the walls of the abdomen and the sides of the pelvis.

Upon tapping the bladder, about a quart of urine, of light-yellow color, was drawn off.

So extensive and firm were the adhesions to the surrounding tissues, that the size and shape of the organ did not seem to be very materially altered by the depletion of urine; it still remained an ovoid body, reaching as high as the umbilicus, its summit occupying the left side of the abdomen.

Upon making an incision through the anterior wall of the bladder, it was found to be greatly thickened: should judge the thickness of the walls to be two or three times that of the normal bladder. The inner surface of the bladder presented a peculiar appearance; prominent folds, or columns of mucous membrane, crossing each other in the form of a coarse net-work. This retiform arrangement was due to great thickening and hypertrophy of the muscular fibres. The mucous membrane at the base was thrown into about half a dozen very marked folds or ridges.

Lying across the base of the bladder was a peculiar fold, more than half an inch in height, such as is called by Mr. Guthrie "a bar of the neck of the bladder." This bar-like ridge was about three inches in length, half an inch thick, and more than half an inch in height. Its course was transverse, including the openings of the ureters.

Behind this ridge was a deep *cul-de-sac*. Its transverse diameter was about three inches, and its depth more than one inch and a half. A prominent fold toward the left side of the *cul-de-sac* formed a partial septum, nearly dividing it into two.

More than an inch in front of the bar-like ridge was another ridge, quite as remarkable. It extended transversely over the mouth of the urethra like a thick, strong valve. This uvula, or valve, was formed by a great enlargement of the middle lobe of the prostate gland, and, as before stated, formed a strong band across the mouth of the urethra, and must have tended greatly, during life, to detention of urine, and must also have formed a barrier to the entrance of the catheter.

Upon removing the bladder the prostate gland was seen to be greatly hypertrophied, being four or five times the normal size.

The right kidney showed upon its surface several cysts, most of them of small size. The left kidney had several cysts upon its surface, most of them of considerable size. This kidney was atrophied, being less than half the size of the other. Its tissues seemed indurated, and the functions of the organ must have been very imperfectly performed.

GEO. T. STEVENS,
JOHN V. LANSING,
H. R. HASKINS,
A. VAN DERVEER.

ALBANY, *July* 12, 1869.

PHILADELPHIA, *January* 19, 1870.

DEAR DR. MCNAUGHTON: I have to-day carefully examined the morbid specimens which you have been so kind as to send me for my inspection, and now proceed to give you the result, premising that they consist of the prostate gland, bladder, and kidneys, said to have been taken from the body of the late Prof. March, of Albany. The specimens are in excellent preservation.

The prostate gland is greatly hypertrophied, the lateral lobes being two and one-quarter inches in length, nearly two and one-quarter inches in height, or in their vertical diameter, and one and one-eighth inches in thickness. The enlargement of the two lobes is nearly the same, and their structure is throughout very dense and unyielding. During life the two lobes must have been almost in contact with each other along the middle line, leaving merely a very narrow fissure, or sulcus, one and five-eighths of an inch in height, by one and one-half inches in length. The veru montanum at the bottom of the fissure is greatly elongated, but not materially changed in other respects.

The middle lobe of the gland is represented by a column of indurated substance, much thicker in front than behind, and insensibly lost in the posterior extremities of the lateral lobes. It is three-eighths of an inch in its antero-posterior direction, and nearly five-eighths in the vertical.

Immediately behind and adjoining the third lobe is the bar of Guthrie, of a crescentic shape, the concavity being directed backward. It is two inches in length, three-eighths of an inch in the antero-posterior direction, and one-quarter of an inch in depth.

Behind Guthrie's bar, at the bas-fond of the bladder, is a *cul-de-sac*, of a conical shape, the apex presenting toward the rectum. It is one and one-eighth inches in depth, and from one and one-quarter to one and one-half inches in diameter at its base. It is apparently composed solely of the mucous and serous coats of the bladder. From the bottom of this sac to the upper border of the middle lobe of the prostate gland the distance is two inches.

The bladder, measured along the middle line in front, is six inches in length, and nearly five and one-half inches in width at the middle. The inner surface is remarkably fasciculated in its entire extent, and the muscular coat is throughout very considerably hypertrophied, its thickness toward the neck of the organ being fully one-quarter of an inch, somewhat less above and at the middle. A portion of the peritoneal tunic remains attached to the superior portion of the viscus, and is, so far as I am able to judge, in a healthy condition. The ureters open each by a very small orifice upon the surface of Guthrie's bar, the remnant of each being about the natural calibre.

The right kidney is five inches in length by three inches in width at its widest part, and one and one-quarter inches in thickness, the weight being five ounces and four drachms. The pelvis is somewhat enlarged, but the remnant of the ureter is of the natural size. The structure of the organ does not seem to have materially suffered.

The left kidney is atrophied and distorted; its weight is four ounces and two drachms, its length four and one-quarter inches, its width two inches, and its thickness not quite one inch. It is proper to add that a very small portion of the organ is missing, having accidently been cut away. Its proper structure is much changed—a small cyst exists along its convex border; and the fibrous capsule is quite firmly adherent to the surface below. The pelvis appears to be natural.

Both kidneys are covered with a good deal of fat, especially the left. The ureters are absent.

From what precedes it is evident that Dr. March must have long been a great sufferer. He was probably not able for many years to empty his bladder completely. From three to five ounces of urine must, I should suppose, have been habitually retained; and it must have been only with very great exertion that he ever relieved himself at all at any time. As to the passage of a catheter, such an operation might have been possible, on the assumption that all things are, humanly speaking, possible, but that it would have been attended with extreme difficulty, and been productive of great distress, is, I think, unquestionable. Even if a catheter could have been inserted, it would not have been possible to draw off all the urine, or even, probably, any considerable portion of it, owing to the peculiar condition of the prostate gland and the bas-fond of the bladder. The only reasonable hope of relief during life, in my opinion, would have been the perforation of the middle lobe of the prostate gland, an operation which would have been quite practicable, judging from the inconsiderable thickness of its posterior attached border, if this condition could have been satisfactorily ascertained, and the patient had been willing to submit to it. The bladder might readily have been reached through the rectum.

With kind regards and best wishes, I am, very truly your friend,

(Signed) S. D. GROSS.

Prof. McNAUGHTON.

FURTHER RESEARCHES ON THE EFFECTS OF LIGHTNING-STROKE.—In the JOURNAL for November, 1869, we quoted a portion of a very interesting lecture by Mr. Richardson on this subject. The extract, which is now made from the same source, *Medical Times and Gazette*, will be found of equal interest and instruction:

Since we last met there have been several recorded cases of death by lightning, which in a striking manner illustrate some of the facts that have passed before us here in our experimental essays. Among other cases, I notice one which has recently occurred in Belgium. A monk, engaged with his brethren in the fields carrying hay, was leading the horses drawing one of the loads, when, an hour of prayer having come, he kneeled by or near to an iron fence. While in this position he was struck by lightning, and was killed instantly. On his body were found several of the characteristic marks of an electrical stroke, such as we have seen to follow upon the discharge from the Leyden battery. But the mark which caused the greatest wonder was an arborescent mark. The exact figure of a tree, says the narrative, was left on the body, the branches being defined in the most perfect manner, to the minutest ramifications.

We know now what the meaning of this appearance is: that it is the definition of venous trunks, as Beccaria first and correctly taught.

The mention of these marks on the body leads me to make one other preliminary observation respecting the metallic stains or markings which it is said have been seen on the body after lightning-stroke. I maintained in a previous lecture that these markings are ecchymoses, and this is often true; but I should like to supplement that observation by repeating the further observation that they may be true metallic lines, when the metal on the body which has been struck is sufficiently thin to be fused by the current of force which vibrates through it.

We will prove this proposition by experiment. We will take a foot which has been removed from a young pig immediately after the animal was killed and dressed. The skin is beautifully white and delicate. Around the foot we will lay a thin gold wire or chain, and then discharge from the large Leyden battery through the wire. This done, I pass you round the foot, and you will see distinctly, not only a line marking what was the position of the wire, but the metallic gold, inlaid as it were in the skin. We may accept, therefore, that the vulgar statements of the finding of metallic lines on the bodies of persons struck by lightning are sometimes true.

I failed before to illustrate this fact because the wire was too thick, and conveyed the discharge without being fused. And here there is suggested to us a very important piece of knowledge bearing on medico-legal investigations. Persons are sometimes found dead after lightning-storms, and, it may be, a doubt arises respecting the cause of death, or, if the cause be clear as from lightning-stroke, respecting identity of person. In such a case the detection of a metallic mark on the body would be of the greatest moment. The portion of skin containing the mark may be carefully dissected out, and from the color of the stain an idea may be obtained as to the metal that has left the stain. Moreover, by subjecting the part to chemical analysis, the precise metal may be determined. Armed with these two facts, the medical witness could not only say that the mark he found on the body was a true metallic mark, which nothing except electric discharge could strike on the body, but he could further say that the deceased, at the time of being struck, wore or carried a certain metallic substance, of gold, silver, copper, or other metal. In some measure, too, the character of the stain would determine the intensity of the discharge which produced it, and the thickness and substance of the metal which had been fused. A discharge of low intensity might produce heat of the metal and a burn, but it would not fuse even a very thin metal in such a manner as to leave a true metallic stain. A discharge of great intensity, such as we produce with the cascade battery, would produce no stain and probably no mark at all; in other words, it would not be diverted from its course, but would penetrate directly through the organism. The discharge which effectually produces the stain is that which we obtain from the common Leyden battery.

When gold or other metallic substance is fused by the discharge in contact with the natural coverings of animals, such as fur, hair, or feathers, the metal seems to combine with the parts so intimately as to form a part of the structure. In the fur of the rabbit we saw, at last lecture, the gold producing a kind of natural pigment, so that one might say, looking at the animal which was originally white, that it was now gray and white: the color lasts, too, until, by new growth, the fur is replaced. If, therefore, we cannot turn a black white, a process which may not in time be so difficult a task after all, we can turn a white to grayish black with perfect readiness.

An exceedingly pretty experiment in this same direction may be made with the feathers of birds. Here is a feather of exquisite whiteness; I have woven through it some thread of gold: here is another similar feather through which I have woven thread of silver. I will ask our friend Mr. Pepper to pass the Leyden discharge through these metals interwoven with the plumage. He does so, and now you see the feathers are entirely changed in color, variegated in wavy lines. The feather interwoven with gold is of purple tint in the colored parts; the feather interwoven with silver is of silver-gray, like the feather of a gray parrot, and so firmly is the metal implanted it could not readily be removed except by chemical action, which would break up the structure. Vitruvius, telling of the interweaving of gold in the tissues used for dresses by the rich dames of antiquity, informs us that, in order to preserve the gold, the dress, when it was no longer wearable—when it was tattered, we may presume, or out of fashion—was put in an earthen vessel and burnt over a fire. Then the ashes containing the precious metal were carefully gathered together and put into water, where they were treated with quicksilver, which picked up the gold, and converted it into a mass which in these days we call an amalgam, which amalgam, he says, was afterward made to yield back the gold by compression through a piece of cloth. By our modern science we could even be more economical than the artisans of the time of Vitruvius, for we could mark cloth with gold, using grains where they used ounces, and still retaining the power to get our gold back again when we desired. The amalgamation of metallic substances with organic bodies by electrical discharge, such as we see in the feathers before us, opens up a curious physiological question—viz., the part which metals, in a state of very fine subdivision, play in effecting the coloring of many structures of living animals. Here is a red feather plucked from one of the birds called the Turaco. The researches of Mr. Church have proved that this feather contains the metallic element copper, and he has produced for us from it a red coloring matter, which he has named Turacine, and which contains nearly six per cent. of copper. In Turacine the metal copper is a true chemical combining element—as perfect a combining element as sulphur and carbon in albumen.

Cause of Death from Electrical Shock.—Many theories have been advanced to account for the cause of death from lightning and electrical shock. The view held by John Hunter was, perhaps, the earliest that was received, and has been the one most firmly maintained. This view is, "that death is instaneously produced in the muscles, which therefore cannot be affected by any stimulus, nor consequently by the stimulus of death." The view thus so singularly, and, as it would seem to those who have not tried to understand the meaning of Hunter, so enigmatically expressed, is based on the notion—believed by its author to be a fact—that muscles do never contract after death by electricity. This idea we have seen not to be based on fact, and the theory founded upon it falls altogether to the ground.

Other authors have supposed that in all cases of death the fatal result is due to mechanical injury—to rupture of some structure or structures which are essential to the continuance of life, and the integrity of which cannot be broken without a fatal catastrophe; while a third class have attributed death to spasm of the heart and of the muscles of respiration.

It seems to me, from the experiments we have made in this place, that strictly none of these views are correct, but that the immediate cause of death is of a more simple nature. I trace the cause of death in all cases, where it is instantaneous, to the process of sudden expansion of the gaseous part or atmosphere of the blood, combined in extreme degrees of shock with a sudden conversion of animal fluid from the fluid into the gaseous condition. Let me take an egg in illustration. I will make a pin-hole at each extremity of the egg, and pass the wires from the Leyden battery, positive and negative, one into each hole. I will next discharge the battery, and, from the expansion which occurs, the shell of the egg is torn into fragments, and it, with what it contained previously, is cast about many feet, I may say yards, in all directions. Let me modify this experiment. I take the head of a dead fowl, and dissect back the skin from the crown of the head. Then I connect the medulla with the negative pole of the battery, and, having charged the battery, I direct the positive pole toward the exposed portion of skull, and make the discharge. See what has happened. Although, from the fact of the head being removed from the body, there is an opening by the spinal canal into the skull cavity, yet, such has been the expansion, the skull is not merely fractured, but a portion of it, of the size of a sixpence, is lifted off entirely, and the brain is exposed to view. By this experiment we see what is the instant effect of electrical discharge on the animal body. In proportion to the power of the shock there is expansion within the organism. There is expansion of the blood, distention of vessel, and, according to the degree and place of expansion, there is infliction of serious injury.

In reviewing the character of the injuries caused by the electrical discharge, we see clearly the simple effects of expansion, and, by this key to our readings, we are able to understand every lesion. The arborescent markings of external veins, the great distention of the large veins and of the right side of the heart, the rupture of the right side of the heart in one of the cases, the serous effusions, the ecchymosis, and the rupture of the stomach, are, one and all, phenomena at once explainable on this simple exposition. Equally clear on the same exposition are the nervous phenomena. The sudden insensibility reaching the insensibility of death is the result of sudden expansion within the closed cavity of the skull—expansion by which the molecular construction of the nervous mass is disturbed, disarranged, and irrevocably broken.

Nay, more, by this same exposition we are able to understand the phenomena following upon those minor shocks which we have seen producing temporary insensibility, and what, in a certain sense, may be called anæsthesia. In these examples the structure of the brain is disturbed, but not irrevocably broken; there is induced a loss of function, but, by time and rest, the order is restored, the different minute parts, by attraction or cohesion, come again together, the centres are reformed, and, with the restoration of natural structure, there is restoration of natural action.

By this same exposition we may also readily account for those local lesions and secondary effects of local lesions which we have traced as following after electrical shocks and lightning-shocks that have not been sufficient to kill—perhaps not sufficient at the time to lay the body prostrate and insensible. The paralytic state, the catalepsy, the epilepsy, may all be traced to pure disarrangement of nervous matter from the one physical cause, *expansion.*

A last and extreme condition yet remains to be thought of, and that is the complete disorganization which has been observed after lightning-stroke; when the blood is left fluid and incoagulable, the body distended with gas, and the destruction of organic parts wellnigh universal. Once more, in exposition of these conditions, expansion, due to vehement force of stroke, is sufficient to account for all the phenomena. The blood, in such cases, as Dr. Cleveland has ably pointed out, is decomposed throughout the body, as it is in our closed tubes when we send through them, charged with blood, the Leyden shock: the color of the blood is changed to deep black, and gases of decomposition are instantly set free, while in a greater or lesser degree the same change extends in the soft solids. In a word, there is general decomposition.

Treatment after Electrical or Lightning Stroke.—When an animal has received a lightning-stroke or an electrical shock which at one shock paralyzes the heart, the death is the most complete and sudden of all deaths; nothing, indeed, admits of being done. But in many cases the action of the heart continues after the respiration has ceased and after complete unconsciousness, and it might be expected in these instances that some line of treatment would be possible which should restore respiration and the other functions of life. I regret that in most cases every means which may be devised will fail; but there are three methods of recovery which have been employed, and all of which have been tested by experiment on the inferior animals with varying success. I will notice them in what seems to me the order of their value.

Venesection.—The free abstraction of blood is the first and best treatment after lightning-shock. In one of the cases related in a former lecture, where several men on board ship were struck by lightning and were rendered insensible, it will be remembered that free bleeding was employed with the utmost benefit, and that from one man no less than fifty-two ounces of blood were drawn. This practice is in entire accord with principle. At a moment when all the large veins and the right cavities of the heart are tense almost to the point of rupture, and when the brain is bearing a compression which causes unconsciousness, what can be more reasonable than to take off the distention by making a free opening into the circulatory system? Presuming even that the heart has ceased to beat, the opening of a vein can do no harm, because the vein will fail to bleed, and therefore no blood will be lost; presuming that the heart has not ceased to beat, but is failing from the pressure which is being exerted upon it, then the least removal of that pressure is the surest mode of setting the organ free. We have had here some most unexpected and important evidences of this truth. We find that if a sheep, which is about to be slaughtered, be first struck down by the discharge—by the discharge from the cascade battery, or by the discharge from the ordinary Leyden battery of one hundred square feet, superficial—the animal, on receipt of the discharge through the body, falls as if dead, and is in fact dead. But if now, instantly, the throat be transfixed by the butcher, in the usual way of killing sheep, in some seconds after the blood begins to flow, the animal shows what would appear to be the signs of recovery—that is to say, the respiratory movements are renewed, and, before the actual death, there are the general convulsive movements which follow death from simple hæmorrhage.

Experience and experiment thus combine to teach us that the first step to take, if called to a person stunned by lightning, is to draw blood from a vein. I should myself be inclined to draw from the external jugular vein, and to lay the body perfectly horizontal, so as to get an easy outflow.

Artificial Respiration.—Artificial respiration, has not, as far as I am aware, been tried on the human subject after death by lightning-stroke. We have tried it here in several cases on animals which have been struck down by the discharge, and I have devised for these experiments a pair of pocket-bellows, which leave nothing to be desired in respect to the production of a perfect and accurate imitation of natural respiration. The bellows are constructed really on the plan of the bellows invented originally by John Hunter, reinvented by M. Gorcy, of the Military Hospital of Neufbrisac, in 1790, under the name of the "apodopnic" bellows, and again reinvented, about 1835, by Mr. Read, of Regent's Circus, Piccadilly. They are, as I have modified them, different from those which preceded, in being constructed of india-rubber, and in being exceedingly portable and manageable. By one grasp of the hand with these bellows we fill the lung of the animal, through one nostril, with fresh air, and by relaxation of the grasp we extract the impure air from the lungs—the bellows having two chambers, one for feeding the lung with pure air, the other for removing from the lung impure air.

But, despite all this care to make the process of artificial respiration practically perfect, we have found it by itself of no real efficacy in treating animals which have been struck by electrical discharge. I will make here an experiment in point. With one discharge from the Leyden battery we will strike down a large rabbit. I find the heart of the animal is still beating, and at once, by inserting the bellows-tube in the nostril and setting the bellows in gentle action, I induce artificial respiration. See, now, how accurate is the representation of the natural act of breathing; you might conceive that the animal was alive. But I keep this action up for any time without result; the nervous centres are under pressure, or are disorganized by separation of part, and the heart is under pressure which it will struggle to oppose in vain, until it becomes paralyzed by its own efforts to lift its column of blood. I do not, therefore, attach any importance to artificial respiration as a primary method of restoring animation after lightning-shock, but I think it would be an admirable adjunct to treatment by free venesection. Directly the heart were set at liberty from its bonds, the contraction of the right ventricle, effectively made, would send a wave of blood into the pulmonary circuit; and if then that charge of blood should be allowed, by means of artificial respiration, to meet a charge of fresh air, the best results might naturally be anticipated. I would venture to lay down, as the second important rule in all cases of shock by lightning, that, so soon as a vein can be got to yield blood, the lungs should be gently and steadily charged and emptied of air by the action of a double-acting bellows, such as I have used to-day.

Electrical Excitation.—Paradoxical as it may seem, it is nevertheless true that electrical excitation has been proposed as a means of recovery from electrical and lightning shock.

Mr. Kite, a writer of no mean pretensions in the last century, urged this practice very earnestly, and the Royal Humane Society, in its report for the year 1785–'86, inserted a letter from one "Humanus," of Bristol, supporting the same view. Among other arguments used by "Humanus" was the narrative of a case in which the practice was actually and, he thinks, successfully employed. On Thursday, June 18, 1782, he relates that a house in Gravel Lane, Southwark, was struck by lightning, and an elderly man was thrown with violence from his chair and taken up for dead. In this hopeless state "electrization was performed by a skilful practitioner of Guy's Hospital, by which remedy the man was at length entirely restored." The most potent advocate of this method of treatment, however, was Dr. Abilgard, a French physician, who corresponded with Benjamin Franklin, and won from that philosopher great admiration. Abilgard's theory was founded on experiments on fowls, in which he showed that, after fowls were apparently struck dead by violent electrical shock passed through their heads, and were not recoverable by ordinary means, they were soon reanimated by gentle shocks passing through *the heart and lungs.* To some extent there is a reason, I find, in this argument. If the electrical discharge be passed purely and simply through the brain of an animal, it produces a concussion which strikes the animal senseless, and does not necessarily kill. In this state the heart remains in motion, and, if the respiration be sustained, either by artificial means or by exciting the respiratory muscles, recovery will sometimes, especially in young animals, occur. At this point we must "season our admiration" for the use of electricity *versus* electricity. In an animal or man struck down by lightning, the injury is not specially localized to the brain; the heart shares in the disturbance, and the transmission of the electrical shock through the organ would be more likely to lead to disaster than to good. We have tried here electricity as a restorative from electrical shock through the body, in every practicable manner, and, although we can often excite muscular action even for an hour after death, we have not succeeded in doing more than produce contractions with each stroke administered, and by which the natural power of contraction, under excitation, resident in the muscles, has definitely declined. On the whole, therefore, at this stage of thought, I should rather discountenance the practice of electrical excitation as a remedy for shock from lightning or electrical discharge.

ACCIDENTS CAUSED BY LIGHTNING. — "The characteristic effects of lightning," says M. Boudin (*Gazette Médicale de*

Paris, July 31, 1869), "are their suddenness, their contrast, their opposition, and their mystery." This receives a new confirmation in the interesting fact communicated by M. Tourdes to the Academy of Sciences: The lightning fell on a chestnut-tree, near the bridge of Kehl, under shelter of which were three soldiers. They were all prostrated; one died instantly, the second within a few minutes, the third survived. Other persons more or less distant on both shores of the Rhine experienced the effects of the *choc en retour*. In the first place, what will account for the difference presented by the three military men in resisting the action of the lightning? We know that all individuals do not possess the same conductibility for electricity; some, according to Boudin and Arago, can stop on a sudden the communication of an electric chain without receiving the least shock. Statistics are agreed upon the fact that women are less often struck by lightning than men. Let us see now the lesions presented by the soldiers: "These lesions," says M. Tourdes, "are cutaneous, external, and superficial; they consist in burns with erosion, destruction of the epidermis, scorching of the derma, formation of patches like parchment. One foot only exhibited phlyctenæ on the plantar surface. The burns had three forms: punctated, in patches, or in long furrows. The two men who succumbed had the hair, eyebrows, eyelashes, and mustache burned. All three presented a scrotal lesion. No mechanical lesion can explain the death of the two victims; the anatomical characters were those of asphyxia, less marked in the individual instantly killed. The membranum tympani was burst in one case. The rigor mortis came on very quick; the blood was brownish and liquid, without clots; it exhibited neither crystals nor any deformation of the globules further than what corresponded to the time of death."

DIFFERENTIAL ANATOMY OF DIPHTHERITIS AND CROUP. BY E. WAGNER.—According to Dr. E. Wagner (*Gazette Médicale de Paris*, July 17th), diphtheritis is an affection of the mucous membrane, in which this membrane is more or less infiltrated, thickened, and covered over by a grayish formation which is closely adherent to it. In croup, on the contrary, the mucous membrane seems to be about normal to the naked eye, or simply congested and lined by a slightly-adhering membrane.

In diphtheritis the adventitious membrane presents itself as a clear, homogeneous net, the meshes of which are either empty or filled with various elements. The limits between the deep portion of the new formation and the mucous membrane are always distinct, although their adhesion is very firm. The diphtheritic net is formed at the expense of the epithelium. Through a special metamorphosis of the epithelial cells this metamorphosis presents three stages: 1. Increase in the size of the cells; 2. Formation in the protoplasma of clear spaces, which gradually increase in size by trespassing upon the cellular contents which become jagged; 3. Continuation of the same process and complete extinction of the nucleus. Each cell is then replaced by a reticulated, jagged, perforated formation, the branches of which attach themselves to the branches of the neighboring cells to constitute the diphtheritic formation. Chemically speaking, the membrane of croup and that of diphtheritis cannot be distinguished; they strongly resemble coagulated fibrine. The diphtheritic membrane possesses a remarkable unalterability by the ordinary chemical reagents.

The croupous membrane consists of a finely-woven net, the meshes of which contain a large amount of elements quite similar to purulent globules. These globules are much more numerous than in diphtheritis. The formation of the two membranes is identical; in croup the higher fineness of the reticulum is due to the more active production of globular elements in the interior of the epithelial cells, while in diphtheritis the production of globular elements is very limited. As to the connection of the croupous membrane to the mucous structure, the slight adherence is owing to the interposition of a thin layer of muco-purulent liquid which separates them.

Before closing, the author concludes as follows: "There is no definite limit between croup and diphtheritis. Both are met as well in the pharynx as in the respiratory tract; but, in fatal cases, diphtheritis affects, by preference, the pharynx; croup, the inferior portion of the larynx, trachea, and bronchi; while the superior portion of the larynx offers a combination of the two affections, or rather an intermediate form."

TRICHINIASIS.—De Kalb County, Illinois, has been in an extreme excitement about the appearance of this disease, which, however, has been confined to one or two points, and in all cases was clearly traceable to eating diseased pork in a raw or improperly-cooked state. In Marengo, out of a family of ten persons, eight were attacked with a disease resembling typhoid fever, except that there were swelling and pain of the voluntary muscles. The attending physician quickly traced the origin of the disease, and found the trichinæ in large numbers in the food of which the family had been partaking. Three of the cases had proved fatal, and but little hopes were entertained of the recovery of some of the other patients.

Perhaps a bit of the wholesome legislation which is applied in Germany, to prevent these sad occurrences, might avail in this country. An instance in point recently happened in Jena, where two young men died after eating pork, and, as the animal had not been subjected to a microscopical examination, as required by the law, the proprietor was punished with two months' and the butcher who sold the pork with four months' imprisonment.

"Housemaid's Knee," it is said, in the foreign journals, is now called by the London surgeons the "Ritualistic Knee," out of compliment (?) to the High Churchmen whose costumes and genuflexions have latterly been the subject of so much comment, both favorable and unfavorable.

Professional Quackery—A Good Example.—We have received the following communication from the Hennepin County Medical Society, Minn., and take occasion to commend its action to other organizations:

Dr. Geo. F. Shrady:

My dear Sir—I send you a copy of a preamble and resolutions adopted by this Society at its last stated meeting, holden November 6th. It may interest you as being the direct result of your editorial of a month since, entitled "Professional Quackery." Very truly yours,

W. F. Hutchinson, M. D.,
Secretary.

Minneapolis, Minn., *November* 9, 1869.

Whereas, Action of this body has been deemed necessary in order to prevent professional quackery in the matter of advertising surgical operations and in the daily journals:

Resolved, That any member of this Society who shall permit his name to appear in connection with a report of a surgical operation or case of disease in the public prints, or who shall furnish any secular journal with such report for publication, shall be deemed guilty of gross violation of the medical code of ethics and of professional honor.

Resolved, That it shall be the duty of the Committee on Ethics to investigate each case as it appears in print, and report the result to the Society at its next regtlar meeting; and the offender, if found guilty, shall be reprimanded by the President for the first offence, and summarily expelled for the second.

Resolved, That the Secretary be instructed to transmit a copy of this preamble and these resolutions to each member of this Society, to each newspaper published in the city, to the Secretary of the State Medical Society, and to the Editor of the *New York Medical Record.—Medical Record*

A NEW medical society has been established in Washington, to be called the "National Medical Society of the District of Columbia." By the constitution, right of admission is granted to women and colored folk. Indeed, it seems that a charter could not have been obtained from Congress on any other terms.

THE Messrs. Galignani of Paris, proprietors of the journal known by their name, established, in 1865, a small hospital exclusively for the English poor in Paris. Since its opening in October of that year, it has received and cared for three hundred and fifty-one patients, and has been the means of relieving a large amount of distress among the English inhabitants of that great city. On account of the small size of the hospital—it contains but twenty beds—none but serious cases are admitted.

PROF. J. D. RANKIN, of the Galveston Medical College (*Galveston Medical Journal*), objects to the classification of malarious fevers into "*Intermittent*, *Remittent*, *and Congestive*," on the ground that these titles are not sufficiently expressive of the morbid conditions; he therefore proposes the simpler and more intelligible names: "Passive Hemato-Neuro-Endangitis Gravis! Hemato-Neuro-Endangitis Gravior!! and Hemato-Neuro-Endangitis Gravisimos!!!" It is the characteristic of true genius to simplify all it touches.—*Medical Gazette.*

ERRATUM.—The case of external perineal urethrotomy, reported in the last number of the JOURNAL, should have been credited to Dr. W. A. Bradfield, of Uniontown, Alabama, instead of to Dr. Coleman. The mistake arose from the fact that the author's name was not attached to the manuscript; and, the letter which originally accompanied the manuscript having been mislaid, we were compelled to rely upon the best evidence we could obtain from our subscription-books in tracing the authorship. No one will probably be more surprised than Dr. Coleman at the honors we thus thrust upon him, or more prompt to repudiate them.

On Relapsing Fever.—A Lecture by AUSTIN FLINT, M. D.,[1] Professor of the Principles and Practice of Medicine, and of Clinical Medicine, in the Bellevue Hospital Medical College.[2]

GENTLEMEN: Some weeks since, when I took up in order (as I have done for the last nineteen years), in my didactic course, Relapsing Fever, I stated that there were cases then under observation, at Bellevue Hospital, which presented the characters of that disease. I had not then observed the cases sufficiently to feel quite sure concerning the nature of the disease. I afterward stated that there could be no longer any doubt as to the correctness of the diagnosis, and promised to devote to the disease some further consideration. I propose to redeem that promise in my lecture to-day.

Let me first name the publications which you may consult for facts, chronological and geographical, relating to the past prevalence of this disease; and also, for a fair exposition of the knowledge contained in medical literature respecting it. I shall content myself with doing this, for I do not wish to occupy your time with historical details which you can, if you choose, glean from books at your leisure.

You will find this disease treated of concisely in most, if not all, of the late works on the Practice of Medicine. It is considered fully in Murchison's excellent treatise on the continued fevers of Great Britain. In the *British and Foreign Medico-Chirurgical Review*, number for July, 1851, you will find a very able article on the Continued Fevers, including Relapsing Fever, with numerous bibliographical references. A list of successive epidemics, in Scotland, Ireland, and England, is given in that paper. Dr. William Jenner, in addition to an account of an epidemic which he studied in 1847, submits facts establishing the non-identity of relapsing with typhus and typhoid fever, in a paper published in the *Medico-Chirurgical Transactions*, vol. xxxiii., new series, 1850.

In this country, exclusive of systematic treatises on the Practice of Medicine, medical literature contains very little relating to relapsing fever. Judging from the little that has been published, the disease, with a very few exceptions, has not heretofore existed on this side of the Atlantic. Dr. Meredith Clymer observed and described fifteen cases, occurring at Philadelphia in 1844.[1] Dr. A. Dubois reported a few cases, occurring in New York, in 1848.[2] I recorded fifteen cases at the hospital in Buffalo, 1850–'51; and I gave the results of the analysis of these cases, together with an account of the disease as described by British writers, in my work entitled "Clinical Reports on Continued Fever, based on an Analysis of one hundred and sixty-four Cases." This work, which I suppose to be now out of print, was published in 1852.

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[1] It is proper to state that this published lecture contains the matter of two lectures, delivered at Bellevue Hospital, February 1 and 8, 1870.

[2] This article was not received in season for insertion among the "Original Communications," but the importance of the subject induces us to insert it here, rather than delay its publication.—[ED. N. Y. MED. JOUR.

The circumstances under which these fifteen cases came under my observation were as follows: In 1850–'51, I was engaged in the study of continued fever. Of forty-eight cases which I recorded and analyzed in those years, following the example of Louis, fifteen were distinguished by the occurrence of a relapse after convalescence had appeared to be established. I was then unacquainted with the distinctive characters of relapsing fever; and, of these fifteen cases, nine were included in the group of cases of typhoid fever, one case was considered as typhus, and five cases were placed by themselves and called cases of doubtful type. The question, whether these fifteen cases were cases of relapsing fever, arose after reading the article in the *British and Foreign Medico-Chirurgical Review* to which reference has been made. I then analyzed these fifteen cases separately, and found that the facts corresponded, in all respects, with the history of relapsing fever. The results of the analysis are given in my "Clinical Reports on Continued Fever," and I shall refer to them, in connection with the clinical history of the disease, in this lecture.

Some of the different names by which the disease is now and has been heretofore known are to be mentioned. In past literature it has been called "five-day fever," "seven-day fever," "short fever," "mild yellow fever." By some of the British writers it is distinguished as "famine fever," and by some German authors as "hunger pest." The name "relapsing fever" is based on the most striking of its peculiarities, and it involves no hypothesis; it is therefore to be preferred to any other, with our present knowledge.

By way of introduction to the consideration of important questions concerning the disease, I will read a report of one of the first of the cases received into the hospital—a case which is typical as regards the prominent, distinctive features of this species of fever; and I will then introduce a patient, who is supposed to be passing through the primary paroxysm, and also a patient who has passed through the disease, and is now convalescing. The report which I shall proceed to read was drawn up by Dr. William M. Polk, senior assistant physician on the third medical division.

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[1] *Vide* Treatise on Fevers, 1841; also, Notes in Aitken's Science and Practice of Medicine. American edition.

[2] *Vide* Transactions of American Medical Association, vol. ii.

Henry Gordon, aged seventeen, a native of this country, by occupation a laborer, was admitted December 17, 1869. He came from No. 37 Mulberry Street, and he stated that several persons in the house in which he lived had been ill with the same kind of fever. He had been exposed to many hardships, inclusive, probably, of a want of proper food.

He was well, up to four days before his admission, when he was seized with a chill, followed by fever and pain in the limbs. He had no epistaxis, nor diarrhœa. He suffered much from headache. On his admission the pulse was 110. He then complained of severe muscular pains. The bowels were constipated. The skin was hot, but moist. There was no eruption; there was no jaundice.

December 18th, the day after his admission, he was free from fever. The pulse and temperature were normal. There was profuse perspiration on this date.

He continued free from fever from the 18th to the 28th, that is, ten days. The last four of these ten days he was sitting up. He continued, however, to complain of muscular pains, and he was quite weak.

On the 28th of December, after getting up, at 7 A. M., he felt chilly sensations, and soon returned to bed. The pulse was now 100, and the temperature in the axilla was 100°. At evening the pulse was 110, and the temperature 101°. On the 29th, the pulse was 120 at A. M., and the temperature 102°. He had epistaxis for a few moments on this date, and diarrhœa. The fever persisted without any abatement for the two following days, the pulse being 120, and the temperature 102°. On January 1st, at 9 A. M., the pulse fell to 90, and the temperature to 100°. He was then perspiring profusely. At 6 P. M. the pulse and temperature were normal. Free perspiration continued. The muscular pains had continued to be, and were still, severe.

The relapse of fever thus ended after a continuance of five days. After January 1st there was no return of fever. On the 13th of January he was able to sit up. He convalesced without any drawback, and was discharged, quite well, January 24th. There was no eruption of any kind. The diarrhœa continued only two days, and was slight.

The treatment was as follows: Ten grains of quinine were given daily, for the first four days, in one dose, at noon; and two grains three times daily, during the intermission. Dilute nitric acid was given during the continuance of fever. Bismuth, kino, and opium, were given to relieve the diarrhœa.

The points in this case to which your attention is to be directed are, the abruptness of the attack; the continuance of high febrile movement for five days; the sudden and complete disappearance of fever, profuse perspiration occurring at the same time; an apyrexial period of ten days; an abrupt relapse of fever lasting five days, the second febrile career ending suddenly, with profuse perspiration, and convalescence then taking place; the prominence of muscular pains, both during the fever and in the intermission; the residence of the patient in a house where this fever existed; his suffering from deprivations before being attacked; and, finally, the absence in the history of the characters which distinguish severally typhus, typhoid, and remittent fever.

I shall now introduce a patient who is supposed to be passing through the primary febrile career of relapsing fever. The patient is a girl fourteen years of age. She was admitted with fever, which has now continued for five days. The pulse is 140, and the axillary temperature 101°. Now let us consider the question, What are the grounds for supposing this to be a case of relapsing fever?

In the first place, it is an essential fever of some kind, for a careful examination of the different organs of the body has failed to disclose an acute inflammation anywhere; it is not, therefore, a symptomatic fever. It is not one of the eruptive fevers, for there has been ample time for the eruption and other diagnostic characters of these fevers to become manifested, and they are wanting. If not relapsing fever, it must be a febricula, or remittent fever, or typhus, or typhoid fever. The duration and the intensity of the febrile phenomena show that it is not a febricula. None of the characters of remittent fever have been exhibited. The abruptness of the attack, and the absence of the abdominal symptoms of typhoid fever, warrant us in saying that it is not, in all probability, that disease. Finally, were the disease typhus fever, it is time for the eruption to have appeared, and this is rarely wanting is cases of typhus. The eruption is not present in this case; moreover, there is not the dusky hue of the face which characterizes typhus; nor is there the physiognomy representing the mental condition which belongs both to typhus and typhoid fever. There has been no incoherency or passive delirium, and the patient is now able to give an accurate account of the past and present symptoms.

Thus, gentlemen, you see that the diagnosis is reached, at the present time, in this case, chiefly by exclusion; but, if the disease be relapsing fever, we may expect daily a sudden cessation of the febrile symptoms, and a period of apyrexia, or an intermission, which will probably be followed by a relapse. The diagnosis will then be confirmed.[1]

Before this patient is removed, let me call your attention to the presence of petechiæ on the lower limbs. The small, round, dark spots which you see are truly petechial, that is, they are minute extravasations of blood, or ecchymoses. They may occur in relapsing fever, as in typhus or typhoid fever, and also, in various diseases. They have no diagnostic significance, nor do they denote an unusual gravity of disease.

I now introduce a patient who is convalescing from relapsing fever. The first career of fever was seven days in this case; then followed an intermission of seven days. The relapse of fever lasted only four days. He is able to walk up to the amphitheatre, and is nearly well enough to be discharged.

The remainder of my lecture I shall devote to a brief consideration of the following questions: What points in the clinical history of relapsing fever are distinctive of the disease; and has it any anatomical characteristics? What are the grounds for considering this disease as a distinct species of fever? What are the points involved in the diagnosis of relapsing fever? What is the existing state of our knowledge of its causation? What is the prognosis? and, lastly, what are the indications for treatment? These questions I shall take up *seriatim*, following the order in which they are stated.

What points in the clinical history of relapsing fever are distinctive of the disease; and has it any anatomical characteristics? My remarks under this head, as well as in relation to the other questions, will be based, not exclusively on the information to be obtained from the writings to which reference has been made, but on the observations in this hospital during the present prevalence of the disease. Through the zeal and assiduity of the house physician of the third medical division, Dr. Thomas J. Moore, I have been furnished with sixteen recorded histories. These histories have been recorded with care, the pulse and axillary temperature having generally been noted twice daily. I am much indebted to this gentleman for his interest and fidelity. I should add that most of the cases were under my observation. Dr. Moore has also furnished a tabulated statement of statistical facts relating to the cases throughout the hospital.[1] I have carefully analyzed the sixteen cases just referred to. In addition, I have the results of an analysis of the fifteen cases which I observed and recorded in 1850–'51. The two collections thus make thirty-one recorded cases.

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[1] The subsequent history of this case corroborated the diagnosis.

Abruptness of invasion characterizes the disease. The attack is sudden. There is no prodromic period. The seizure is almost always marked by a well-pronounced chill, which is immediately followed by febrile movement. Usually, the patient at once takes to the bed; but, in some cases, one, two, or three days pass before there is this evidence of yielding to the disease. Moderate perspiration occurs shortly after the fever begins, in a considerable proportion of cases. This was noted in seven of twelve cases which I formerly observed. It is noted in the histories of several of the cases recently under observation. The perspiration in some cases is abundant, and it may recur repeatedly during the continuance of the febrile paroxysm.

The fever attains quickly to either considerable or great intensity, as denoted by the pulse and axillary temperature. Thus, of two cases in which the disease was developed in the hospital, in one, the pulse on the first day was 120, and the temperature 103°; in the other case, the pulse on the first day was 130, and the temperature was 103°. During the continuance of the first paroxysm, the pulse and temperature generally denote a persistent intensity of fever, the pulse ranging, in different cases, from 100 to 140, and the temperature from 100° to 105°. The oscillations are rarely great, and those which occur are irregular in their occurrence.

The cessation of the fever is as abrupt as the accession. The pulse and temperature quickly fall to nearly or quite the normal standard. The transition from high fever to complete apyrexia takes place, often, in a few hours; usually this is accompanied by profuse perspiration, which continues for several hours, and even an entire day. Not infrequently, the pulse and temperature fall below the standard of health. In two of the cases recently under observation, the pulse fell to 54, and in one case the temperature to 95°. In a day or two both the pulse and temperature rise again to the normal standard.

The duration of the primary paroxysm is stated to be, in the majority of cases, from five to seven days. Exceptionally, it may be only two days, and it may be twelve days. In the cases which I formerly observed, the average duration was nine days, the maximum being twelve and the minimum six days. Of ten cases, among those recently observed, in which the duration was definitely determined, the minimum was four days, the maximum eight days, and the average duration was a fraction under six days.

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[1] This statement is appended to the lecture.

During the apyrexial period or intermission, the absence of fever is complete. It is incorrect to call this period a remission; the fever does not remit, but it intermits. The average duration of this period is stated to be about seven days; but it may not exceed two or three days, and it may extend to twelve days, or even more. In the cases which I formerly observed, the average duration was five days, the longest being eight, and the shortest three days. Of the cases recently observed, the minimum duration of this period was five days, and the maximum, nine days. The mean duration was a fraction over six days.

The relapse, like the primary attack, is sudden. It is generally ushered in by chilly sensations, but not so constantly by a well-pronounced chill as the first paroxysm. The fever in the relapse quickly becomes more or less intense. The intensity may exceed that of the first paroxysm. But, in the majority of cases, the intensity is less. The relapse also ends suddenly, and in most cases with profuse perspiration.

The duration of the relapse is stated to vary between three and five days. It may, however, be only twenty-four hours, and it may extend to ten days. In the cases which I formerly observed, the minimum was two days, and the maximum ten days. The mean duration was six days. Of the cases recently under observation, the duration of the relapse is noted in eleven. The shortest duration was three days, and the longest was eight days. The average was four and a half days.

It is to be borne in mind that the relapse does not always take place. It was apparently wanting in one of the cases recently observed. On the other hand, a second, a third, and even a fourth and a fifth, relapse have been observed. In none of the cases which I have seen has there been more than a single relapse.

To illustrate the course of the disease, as represented by the pulse and axillary temperature, I will read the daily record of these symptoms in the history of a case which is selected as typical.

The patient was admitted into Bellevue Hospital on the fifth day of the disease:

	PULSE.	TEMPERATURE.
5th day	105	102.5°
6th "	104 A. M.	103.5° A. M.
" "	108 P. M.	103.5° P. M.
7th "	90 A. M.	102.5° A. M.
" "	96 P. M.	101° P. M.

	PULSE.	TEMPERATURE.	
8th day	88 A. M.	103° A. M.	Profuse perspiration.
" "	114 P. M.	104° A. M.	
9th day	60 A. M.	96° A. M.	
" "	68 P. M.	97° P. M.	
10th "	60 A. M.	98° A. M.	
" "	54 P. M.	96° P. M.	
11th "	56	98.5°	
12th "	64	98°	
13th "	64	98.5°	
14th "	100 A. M.	105° A. M.	
" "	105 P. M.	105° P. M.	
15th "	96	102°	
16th "	114	104°	Profuse perspiration.
17th "	66	91°	

The points which have been presented are those most highly distinctive of relapsing fever. It remains to notice certain other points, belonging to the clinical history, which are more or less characteristic.

Of symptoms referable to the digestive system, nausea and vomiting occur sufficiently often to be somewhat distinctive, especially when this disease is contrasted with typhus and typhoid fever. Not infrequently, these symptoms are prominent and persistent during the febrile paroxysms. The matter vomited is green or yellow from the presence of bile. The tongue presents nothing distinctive; it is generally coated, and in some cases becomes dry and fissured. The vomiting of blood presenting the character of "black vomit" has been observed; but it is a very rare symptom, and I have not heard of its occurrence in any case during the present prevalence of the disease. It is generally associated with hæmorrhage in other situations, and it is to be considered as an accidental event, not as an element of the disease. It probably denotes a scorbutic complication. Diarrhœa occurs very infrequently; constipation is the rule. The diarrhœa, when it occurs, is evidently accidental. Meteorism, in a moderate degree, is not uncommon; it existed in nine of the fifteen cases which I formerly observed. In only one case, however, was there any considerable tympanitic distention of the abdomen. Tenderness on pressure over the epigastric region existed in six of these fifteen cases. Slight tenderness in the iliac region is not uncommon. Notable tenderness exists in some cases over the liver and spleen; and enlargement of these organs is sometimes determinable by palpation and percussion. I have not observed a craving for food during the paroxysms, which, according to some writers, is distinctive of this form of fever. The appetite, however, returns during the intermission, and the digestion may be active in this stage.

The occurrence of jaundice may be mentioned in this connection. This event occurs in a small proportion of cases, but its infrequency in the other continued fevers renders it somewhat distinctive of relapsing fever. It was present in two of the fifteen cases which I formerly observed, and in four of the sixteen cases recently under observation. In these six of thirty-one cases the jaundice was slight, and the cases in which it was present were not unusually severe. It is doubtful if the statement that this event is an element of gravity be correct. The event is much more frequent in some epidemics than in others. The name "mild yellow fever," which has heretofore been applied to some epidemics of relapsing fever, derives whatever pertinency it has from the occurrence of jaundice sometimes in a considerable proportion of cases, and also from the occasional occurrence of black vomit.

A symptom referable to the nervous and the muscular system is highly distinctive of the disease under consideration. I refer to arthritic and muscular pains, more especially the latter. During the first paroxysm, pains in the loins, the calves of the legs, and the muscles in other situations, are generally much complained of. They are never wanting, although, as regards intensity, they differ considerably in different cases. The muscular pains do not cease with the ending of the paroxysm, but they continue during the intermission; they are more or less prominent during the relapse, and they are apt to persist into convalescence.

The mental condition, perhaps, in a measure, accounts for the suffering from these pains. The perceptions are not blunted in this disease as they are in typhus and typhoid fever. This is a negative point of distinction in contrast with the fevers just named. Another negative point is the absence of the delirium which characterizes typhus and typhoid fever. Delirium is by no means absent in all cases of relapsing fever; but the delirium is such as is apt to occur whenever there is high febrile movement, whether the fever be essential or symptomatic, and it is generally manifested only at night. In the daytime the mental faculties are generally intact. The condition known as coma-vigil does not belong to the clinical history of relapsing fever. This statement is also true of subsultus, carphologia, and other ataxic symptoms which occur in grave cases of typhus and typhoid fever. Deafness is also a rare symptom in relapsing fever.

A distinctive point, in comparison with typhus and typhoid fever, is the absence of a characteristic eruption. In most cases there is no eruption. Sudamina or miliary vesicles are sometimes observed at the time when profuse perspiration occurs; but this eruption is incidental to various affections. The same is true of petechial spots which occur in some cases of relapsing fever. Other kinds of eruption are sometimes accidentally associated. Even the rose papules of typhoid fever sometimes occur in this as they do in various other diseases.

•The physiognomy presents nothing distinctive. The face is more or less flushed, as in cases of symptomatic fever. There is not that degree of capillary congestion, marked especially on the cheeks, which exists in typhoid fever, nor the dingy complexion which characterizes typhus. The expression of indifference, vacuity, or stupidity, which is a notable characteristic of the fevers just named, is rarely observed in relapsing fever. In one of the cases recorded by Dr. Moore, it is stated that, in conjunction with slight jaundice, "the face was flushed, as if there existed erysipelas, excepting the cheeks, the latter being quite pale, and the contrast giving a very peculiar appearance."

The urine in relapsing fever is yet to be studied fully. It appears, however, that, as a rule, the quantity of urine is increased, and the urea is in larger proportion than in health. Great diminution, and even suppression of the urine, however, are sometimes observed, uræmic coma and convulsions taking place, but these cases are, happily, exceedingly rare.

It may be stated, as a point somewhat distinctive of relapsing fever, that there is very little liability to serious complications. In this fact we have an explanation, in part at least, of the very small rate of fatality from the disease. Pneumonia, however, occurred in three of the fifteen cases which I formerly observed; but this complication, in these cases, did not prove fatal. It did not occur in any of the cases recently under observation. Of the latter cases, in three, mild bronchitis was a complication. Diarrhœa and dysentery have been observed to occur not infrequently in some epidemics.

What has just been stated with respect to complications holds true as regards sequels. As a rule, important sequels do not occur. A peculiar form of ophthalmia, which was described as following the cases reported by Dr. A. Dubois, of this city, in 1848, is an exception to the rule. It has been repeatedly observed as a sequel of relapsing fever. The peculiarities of the ophthalmia are described in works which treat of the diseases of the eye. This sequel, however, has not occurred in any of the cases which I have observed. Relapsing fever, when it attacks pregnant women, almost always leads to miscarriage or abortion. The mother almost invariably recovers, but the child, no matter how near may be the end of gestation, as a rule, is either stillborn, or dies shortly after birth.

There are no constant lesions found after death which are distinctive of this disease. The spleen is uniformly more or less enlarged and softened; but this occurs in typhus and typhoid fever. The liver is, also, more or less enlarged, but without any special appearance or structural change. Changes in other organs, which may be found, are due either to complications or to antecedent disease. It is a negative point of distinction in contrast with malarial fevers and with typhoid fever, that relapsing fever is devoid of any known anatomical characteristics.

What are the grounds for considering this disease as a distinct species of fever? Is relapsing fever a special form of disease; in other words, is it a distinct species of fever? I think facts warrant a positive answer to this question in the affirmative. Let us consider briefly the grounds for this opinion:

In the first place, the laws of relapsing fever, as regards the primary paroxysm, the intermission and the relapse, or relapses, are very striking and peculiar. The clinical history, in these respects, has but a remote analogy to the different types of intermittent fever, and to the very rare instances in which a relapse of either typhus or typhoid fever is observed. In respect of the distinctive points now referred to, relapsing fever differs essentially from any other form of fever; it stands alone. Moreover, as we have just seen, there are other distinctive points in its clinical history. That it is a distinct species of fever is a fair inference from the peculiarities pertaining to its phenomena and laws.

In the second place, if it be not a distinct disease, it must be a variety of periodical, that is, malarial fever, or of either typhus or typhoid fever. Now, it may be clearly shown to lack certain characters which are essential to the fevers just named. To prove that it is not a form of periodical or malarial fever, it is sufficient to say that it has prevailed repeatedly in situations where the special cause of intermittent and remittent fever, that is, malaria, does not exist; that it is undoubtedly a contagious disease, and that it is not controlled by anti-periodic remedies. The proof involved in these facts is so conclusive that it is unnecessary to cite further evidence. That it is not a variety of typhoid fever is shown conclusively by the absence of the anatomical characteristics of the latter disease, the so-called typhoid lesions of the small intestine; and also by the absence of the essential features pertaining to the clinical history of typhoid fever. The grounds for the non-identity of relapsing fever and typhoid fever are certainly stronger than for the non-identity of typhoid and typhus fever. The essential points of difference, indeed, are more strongly marked than those which distinguish measles from scarlet fever—diseases which were once considered as identical.

To establish the opinion that relapsing fever is a distinct disease, it is then only necessary to show that it is not a variety of typhus. It is certainly a distinct species of fever, if its non-identity with typhus be proven. The points of difference in the clinical history of relapsing fever and typhus are very marked. A relapse of typhus is exceedingly rare; and, when it occurs, the duration of the primary career of fever exceeds the average duration of the first paroxysm of relapsing fever. On the other hand, the occurrence of a relapse in relapsing fever is the rule, the exceptions to which are as rare as is the occurrence of a relapse of typhus. The characteristic eruption which is nearly constant in typhus never occurs in relapsing fever. The physiognomy and the mental condition in the two diseases present wide points of difference. The fatality, which is considerable in typhus, is comparatively insignificant in relapsing fever. But the conclusive proof is that relapsing fever affords no protection against typhus. Patients who have passed through the former have repeatedly contracted, by contagion, the latter. Murchison cites an abundance of facts exemplifying the correctness of this statement. Hence it is wrong to transfer patients affected with relapsing fever to hospital wards containing cases of typhus. There are no facts showing that the contagion of typhus ever gives rise to relapsing fever. The conclusion is, that each of these two diseases has its own special poison or miasm, by means of which it alone is reproduced; that is, neither of these two diseases is capable of communicating the other. This fact is sufficient to establish the opinion of their non-identity. A fact which is distinctive of relapsing fever, as contrasted with typhus and typhoid fever, is that its having been once experienced does not afford exemption from subsequent attacks. It has been known to attack repeatedly the same person.

What are the points involved in the diagnosis of relapsing fever? The laws of relapsing fever, relating to the primary paroxysm, the intermission and the relapse, are so distinctive that there can hardly be room for doubt concerning the diagnosis after the disease has ended. There is difficulty in discriminating it from other essential fevers chiefly in the primary paroxysm. The differential points in this stage I have already presented in connection with the case introduced in the early part of this lecture. I shall content myself with a recapitulation of these points.

The diagnosis involves certain distinctive characters which belong to the primary paroxysm; but it is to be based, more especially, on negative points; in other words, on reasoning by way of exclusion. The distinctive characters are the abruptness of the invasion, the rapid increment of fever, the frequent occurrence of moisture on the skin, or perspiration more or less abundant, without any marked abatement of the fever, the prominence of muscular and arthritic pains, and, in certain cases, the occurrence of jaundice. These are the positive points which are diagnostic of relapsing fever. The fevers to be excluded are the eruptive fevers, febricula, remittent fever, typhus and typhoid fever. Scarlet fever and measles are readily excluded by the absence of the eruption and of the characters which are distinctive of these fevers during the period of invasion. Small-pox is excluded after the third day by the continuance of fever and the absence of the eruption. It is not always practicable to decide at once that the disease is not a febricula; but the intensity of the fever and the notable muscular pains do not belong to the latter. Doubt, however, is soon removed by the persistence of the fever. Typhoid fever is excluded by the absence of a prolonged access, and the absence of the abdominal symptoms, before sufficient time has elapsed for the appearance of the eruption, and of the characteristic mental condition. Typhus is excluded by the absence of the eruption, which appears earlier than in typhoid fever, by the absence of the dusky complexion, and by the absence of the mental condition—the latter also being apparent earlier than in typhoid fever. Remittent fever is excluded by the absence of remissions, these being determinable by a notable diminution of the axillary temperature in some cases in which they are not distinctly denoted by the pulse. Of course, the prevalence of relapsing fever is taken into account in arriving at the diagnosis.

What is the existing state of our knowledge as regards the causation of relapsing fever? Is relapsing fever communicable from the sick to the well? Undoubtedly this question is to be answered affirmatively. This opinion rests on facts derived from the different sources of evidence, exclusive of inoculation, which establish the contagiousness of other diseases; for example, typhus fever. Of those who are attacked during the prevalence of the disease, a large proportion are known to have been brought into contact with, or close proximity to, patients affected with it. The disease is diffused in hospitals among fellow-patients and those who have charge of the sick. During the period in which cases were received in Bellevue Hospital, after the disease began to prevail recently in this city, namely, between November 14, 1869, and February 6, 1870, twelve persons contracted the fever in the hospital. These twelve persons were especially brought into contact with patients affected with the disease, and in no instance did it attack one who had not been thus exposed. One of the senior assistant physicians residing in the hospital has had it. The orderly in one of my wards contracted it; and his wife, who came to nurse him, was attacked by it. The disease has often been diffused in localities in which it did not previously exist, after the importation of a case.

Facts, however, go to show that it is not a highly-contagious disease. Considerable exposure appears, in general, to be necessary. The area of the infecting distance appears to be limited, and it remains to be ascertained whether it may be transported by fomites. Some facts, cited by Murchison, render it probable that it may be diffused in the latter mode. It is especially communicable when the miasm is derived from a number of patients in the wards of a hospital, or in close apartments. The disease is not likely to be contracted from single patients in well-ventilated rooms. The investigations of the Metropolitan Board of Health in this city, during the present prevalence of the disease, have shown that it is diffused chiefly among those living in overcrowded, illy-ventilated tenement-houses. Of the propriety and importance of removing these patients to hospitals or wards devoted specially to cases of this disease, there can be no doubt.

Destitution, deprivations, and especially deficient alimentation, are powerful predisposing causes. Of this fact, the past history of the disease, at different times and places, furnishes abundant evidence. The significance of the names "famine fever" and "hunger pest" relates to this fact. It is a question whether the special poison may not be generated by the want of food and other sanitary deficiencies, irrespective of its production as an infectious miasm. I am not prepared to offer an opinion respecting this question. It is to be remarked, however, that in typhoid fever we have an example of a fever which is, undoubtedly, under certain circumstances, communicable, but which, it is probable, in the majority of cases, originates independently of contagion. For interesting and important details respecting the causation, I refer you especially to the treatise by Murchison.

Statistics do not show any notable etiological influence pertaining to age, sex, or season. As regards age and season, relapsing fever thus differs from typhoid fever.

The causation of relapsing fever is of importance with reference to the inquiry whether the disease is likely to prevail indefinitely in this city, and in other parts of our country. Although its diffusion involves generally, and perhaps always, a contagious principle, the history of the disease in other countries shows that it occurs as an epidemic, and, after continuing for a certain time, it disappears completely. Thus, in the city of London, for the fourteen years preceding the winter of 1868–'69, there had been no cases of the disease. The complete disappearance of the disease for so long a period goes to show the operation of predisposing and coöperating causes in conjunction with the special poison on which the production of the disease depends. In view of the past history of the disease, it is a fair conclusion that it will not become a permanent visitor on this side of the Atlantic. It will prevail in this city as long as auxiliary causes favor its diffusion. It will be likely to be carried to other places, especially to large towns where a considerable portion of the population are living under circumstances favorable to its occurrence; but it may be expected to disappear not long after it ceases to prevail as an epidemic. The effective means for arresting the spread of the disease are, thinning the population of overcrowded tenement-houses, dispersing the occupants of insalubrious dwellings, relieving destitution, especially as regards food, and promptly removing patients to hospitals devoted to cases of relapsing fever.

The prognosis in relapsing fever. It is, at first view, remarkable that a fever of such intensity, and prevailing especially among a class of persons whose powers of tolerance are impaired by previous hardships and want, should have such a small rate of mortality. The mortality, in different collections of cases, varies from two to four per cent. Dr. Moore, in his statistical report, states that, of one hundred and three cases admitted into Bellevue Hospital between November 14, 1869, and February 6, 1870, only two proved fatal.[1] What is the explanation of this low rate of mortality? An explanation, which perhaps is sufficient to account for the fact, has been already given, namely, the disease is very rarely accompanied by any serious complications. The comparatively much greater fatality of typhus and typhoid fever is due mainly to complications. These fevers rarely kill *per se*, that is, death is not purely an effect of the intensity of the disease.

Of the fatal cases of relapsing fever, the death, in a certain proportion, is attributable either to complications, such as pneumonia and dysentery, or to antecedent affections, for example, disease of the kidneys. But this fever may destroy life, irrespective of any important complications or antecedent affection. Several observers have reported cases in which sudden death occurred apparently from syncope. One of the fatal cases in this hospital apparently exemplified this fact. The death was sudden and unexpected during the night, on the seventh day of the disease. At the time of death, the primary paroxysm seemed about to end, free perspiration having taken place. Neither coma nor convulsions occurred, and, as far as information could be obtained, the dying was by syncope. The autopsy revealed no important lesions except that the kidneys appeared to be fatty.

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[1] *Vide* Dr. Moore's report appended to this lecture.

Suppression of urine followed by uræmic coma and convulsions is sometimes a cause of death. This fact was exemplified by one of the two fatal cases occurring in this hospital. It remains to be determined whether the suppression of urine may result from simply functional inactivity of the kidneys, or whether in all cases disease of these organs exists, as either an intercurrent, or an antecedent affection.

What are the indications for the treatment of relapsing fever? There are no known means which can be relied upon for cutting short a relapsing fever. With our present knowledge, there are no remedies which, employed in the intermission, will prevent the relapse. Quinia has been used freely in this stage recently at this hospital, but with no success. Its inutility, as a prophylactic, has been abundantly shown by different observers. Reasoning by analogy, I am led to think that the mineral acids may exert somewhat of that modifying influence, in this disease, which is so manifest in typhus and typhoid fever. They have been, to some extent, used in the cases recently treated here, but not sufficiently to warrant any conclusions based on experience. They certainly deserve to be tried. The treatment, with our present knowledge, must be expectant, meaning by this term, that it is to consist of palliative measures, and those addressed to the particular indications in individual cases.

The intensity of the fever during the paroxysms may be lessened by sponging the body with water, and perhaps by the wet sheet. Cold water may be taken into the stomach freely. Ice-cold carbonic-acid water is an acceptable and useful drink to allay thirst. Cephalalgia may be relieved by cold applications to the head. The bowels should be kept open by saline laxatives. The muscular and arthritic pains call for the use of opium, especially if it produce no unpleasant after-effects. Irrespective of the pains, opiates are indicated to relieve sleeplessness.

The dietetic treatment is important, more especially in the cases in which deficient alimentation has been a predisposing cause of the disease. In this fever, as in other fevers, when alimentary support is indicated, milk is the form of diet to be preferred. And it is to be borne in mind that in this, as in most diseases, there is never any danger of the over-appropriation of nutriment; the only risk is in the ingestion of more food than can be digested. It is desirable that, during the paroxysms, from one to two quarts of milk should be taken daily. In the intermission, when the appetite returns, as much substantial food as can be digested should be allowed; and the more food is appropriated in this stage, the better is the patient enabled to tolerate the relapse. Tonic remedies are indicated throughout the disease, and especially in the intermission. Quinia in small doses, and some preparation of iron, are the tonics to be preferred.

If the symptoms denote asthenia, alcoholic stimulants are indicated. The occasional occurrence of death from syncope renders it important to watch carefully for this indication. When there is any room for supposing that alcoholic stimulants may be useful, they should be given tentatively and continued, or not, according to the effect, especially on the pulse and temperature. They are, of course, urgently indicated if the symptoms denote danger in the direction of asthenia.

Attention to the urine is important. It is to be recollected that a cause of death is suppression of urine. If the urinary excretion be deficient in quantity, or, if, the quantity being sufficient, there be a deficiency of urea, diuretics are indicated. Should the kidneys not respond to diuretics, hydragogue laxatives or cathartics are indicated, with a view to eliminate urea through the alimentary canal. Active hydragogues are, of course, indicated, if uræmic coma or convulsions should supervene.

It is stated that convalescence from relapsing fever is notably slow, patients remaining for a long time much enfeebled, and tolerating with difficulty affections which may occur before the recovery is complete. This has not been a marked peculiarity in the cases which have come under my observation. It is doubtless more likely to be marked in the cases in which there had been impairment of the vital powers from innutrition and hardships before the fever was contracted; and also, it is more likely to be marked where abundant alimentation and the judicious use of stimulants have not entered into the treatment of the disease.

Statistical Report of the Cases of Relapsing Fever at Bellevue Hospital, by THOMAS J. MOORE, M. D., *House Physician.*

BELLEVUE HOSPITAL, *February* 16, 1870.

Prof. AUSTIN FLINT:

Attending Physician Third Medical Division:

DEAR SIR: In addition to the histories previously furnished of Relapsing Fever, I have the honor to submit the following tabulated statement embracing the whole number of cases of Relapsing Fever treated in Bellevue Hospital from November 14, 1869, to February 16, 1870, on the three Medical Divisions of the Hospital and in the Fever Pavilion.

On the First Medical Division.

Total number of cases	24
Number of cases Jaundiced	2
“ “ with Hepatic Tenderness	10
“ “ “ Splenic “	0
“ “ “ Epistaxis	4
“ “ “ Nausea and Vomiting	8
“ “ “ Diarrhœa	6
“ “ “ An Eruption	1
“ “ “ Delirium	2
“ “ “ Bronchitis	6
“ “ Contracting disease in Hospital	4
“ “ with muscular pains in calves of legs, in the knee-joints, and upper extremities	24
Number of cases fatal	1
Cause of death in the fatal case, Suppression of Urine.	
Number of cases with pains in head and back	24

On the Second Medical Division.

Total number of cases	15
Number of cases Jaundiced	2
Number of cases with Hepatic Tenderness	2
“ “ “ Splenic ”	2
“ “ “ Epistaxis	5
“ “ “ Nausea and Vomiting	5
“ “ “ Diarrhœa	2
“ “ “ Eruption	3
“ “ “ Delirium	0
“ “ “ Bronchitis	0
“ “ Contracting disease in Hospital	2
With muscular pains in the calves of the legs, knee-joints and upper extremities	15
Number of cases fatal	1
Autopsy developed Congestion of Liver and Spleen.	
Number of cases with pains in head and back	15

On the Third Medical Division.

Total number of cases	24
Number of cases Jaundiced	4

Number of cases with Hepatic Tenderness	6
“ “ “ Splenic “	9
“ “ “ Epistaxis	9
“ “ “ Nausea and Vomiting	14
“ “ “ Diarrhœa	1
“ “ “ An Eruption (rose-colored, and disappearing on pressure)	1
Delirium	2
Bronchitis	0
Number of cases Contracting diseases in Hospital	5
With muscular pains in calves of the legs, knee-joints, and upper extremities	24
Number of cases fatal	0
“ “ with pains in head and back	24

In the Pavilion.

Total number of cases	40
Number of cases Jaundiced	1
“ “ with Hepatic Tenderness, frequent, but no record kept.	
“ “ “ Splenic “ “ “ “ “	
“ “ “ Epistaxis	3
“ “ “ Nausea and Vomiting	11
“ “ “ Diarrhœa	2
“ “ “ An Eruption	0
“ “ “ Delirium	0
“ “ “ Bronchitis	0
“ “ Contracting disease in Hospital	1
With muscular pains in the calves of the legs, knee-joints, and upper extremities	40
Number of fatal cases	0
“ cases with pains in head and back	40
“ “ “ Convulsions (Epileptiform)	1

Total number of cases treated in Hospital	103
Number of cases Jaundiced	9
“ “ with Hepatic Tenderness	18
“ “ “ Splenic “	11
“ “ “ Epistaxis	21
“ “ “ Nausea and Vomiting	39
“ “ “ Diarrhœa	11
“ “ “ Eruption	5
“ “ “ Delirium	4
“ “ “ Bronchitis	6
Contracting disease in Hospital	12
With muscular pains in the calves of the legs, knee-joints, and upper extremities	103
Number of cases with pains in head and back	103
Number of cases fatal	2
Causes of death in fatal cases, Suppression of Urine	1
“ “ “ “ From the Fever	1
Number of cases with Convulsions (Epileptiform)	1

All cases except those mentioned as having Diarrhœa were more or less constipated.

Very respectfully,
THOMAS J. MOORE, M. D.,
House Physician, Bellevue Hospital.

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Art. I.—*Oxygen Gas as a Remedy in Disease.* By Andrew H. Smith, M. D., New York.

CHAPTER I.

HISTORY.

The therapeutical history of oxygen dates almost from the moment of the discovery of the gas. A few months after he had succeeded in demonstrating oxygen as a separate principle, Priestley discovered its relation to animal life. He found that a mouse confined in a limited quantity of this gas lived at least twice as long as in a like quantity of common air. This fact led him at once to the suggestion that this agent might be usefully employed in cases of disease in which there was deficient vitality. At the same time, the effect of plunging a burning body into oxygen inspired him with a misgiving that oxygen could not be inhaled to any considerable extent without danger of exciting excessive action in the system, that the patient would "*live too fast*"—a phrase which, down to the present day, never fails to rise to the lips of the practitioner to whom this therapeutic measure is suggested for the first time. Thus early were the possible remedial uses of this agent foreseen, and, at the same time, an erroneous idea advanced, which has maintained its hold upon the professional mind, and prevented much good which might otherwise have been attained.

During the fifteen or twenty years following the discovery of Priestley, attention was directed more to the physiological and chemical relations of oxygen than to its use as a remedy. The part played by it in the animal economy was made the subject of investigation by Spallanzani, Fontana, Barthollet, Lavoisier, and others. To Lavoisier belongs the credit of first demonstrating the composition of the atmosphere, and the changes produced in the blood by respiration. Following in his footsteps, Spallanzani showed that the consumption of oxygen was in direct ratio to the muscular activity of the animal. For instance, he found that the chrysalis consumed an exceedingly small amount, the caterpillar a much larger proportion, while the active *imago* demanded a very large quantity for its support.

These researches led to the grand discovery that the new element was the only one, a constant supply of which was necessary for the continuance of life. Food and drink could be withheld for days; even the nitrogen of the atmosphere could be excluded for hours, and yet no serious injury would result. But the animal *began to die* from the instant the supply of oxygen was cut off. No other element stood in this relation to life. No wonder, then, that it was called *vital* air, and that its discovery was thought to have begun a new era for humanity.

The first case in which oxygen was actually employed as a remedy was one reported by Caillens, in 1783. I can find only a reference to this case, which was published in the *Gazette de Santé*. But, in the year following, Jurine, of Geneva, published an essay, in which he cites, at some length, a case of phthisis in a young lady, which was very much benefited by daily inhalations of oxygen. In 1789 Chaptal, of Montpellier, reported two cases of phthisis, in one of which the gas produced great relief while its use was continued, but in the other the effect was not beneficial.

At about this period the French Government desired an expression of opinion from the Academy upon the value of oxygen as a remedy, and Fourcroy was selected to prepare a report. In this report, and in other works which followed, he resigned himself to a current of speculation which drifted him far away from the truth. He saw the effect of oxygen in the action of every remedy, even of muriatic acid, the composition of which was not then known. He claims to have employed oxygen in a considerable number of cases of phthisis, and to have noted a rapid improvement for two or three weeks, after which a violent inflammatory action was set up, and the progress of the disease was greatly accelerated.[1] But his subsequent experiments on animals, in which he describes a state of fever occurring, which eventuated in gangrene of the lungs, lead to the suspicion that the gas which he employed contained some irritating impurity which the imperfect chemistry of the day did not enable him to discover. He became, nevertheless, the founder of a school which interpreted all therapeutical effects by the supposed relation of the agent employed to the oxygen of the system. But it was not until Beddoes began his observations that any valuable practical results were obtained.

In 1789 Beddoes published his book entitled "Considerations on the Factitious Airs." He was at that time Professor of Chemistry at Oxford, but none the less devoted to the practice of medicine, in which he had already attained a high position. To him belongs the credit of being the first to approach the subject without a theory to sustain. It was not until he had accumulated a large number of facts that he attempted to arrange and classify them. His attempts at generalization were not always attended with the happiest results; but the readiness with which he relinquishes a theory the moment it is found to conflict with fact, gives a rare impression of candor and impartiality to his work. The scope of the work includes observations upon several gases besides oxygen, expecially carbonic acid and hydrogen.

His physiological experiments are of great interest. The principal results which he arrived at were the following:

Oxygen produces a remarkable power of resisting asphyxia.

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[1] Annales Chimie, 1789.

It appears that, when the blood contains an unusual amount of oxygen, the animal is better able to support a deficiency of respirable air, or even the presence of an irrespirable gas.

Animals which have respired oxygen resist longer the action of frigorific mixtures.

The action of oxygen seems to be localized principally in the muscular system.

Oxygen is in the highest degree a stimulus to the irritability of the heart and blood-vessels.

The last conclusion is one which succeeding observers will scarcely indorse to the fullest extent. As a stimulant to the circulation, oxygen is certainly far inferior to alcohol; indeed, in many cases, its stimulating effect is scarcely perceptible.

A few isolated cases of success in the therapeutic use of different gases encouraged Beddoes to set on foot the project of a Pneumatic Institute, in which this mode of treatment could be tested on an extensive scale. The plan enlisted the coöperation of Sir H. Davy, who gave himself with ardor to the chemical part of the work, and of the eminent engineer James Watt, whose genius left nothing to be desired in the mechanical appliances for administering the gas. Probably a more brilliant triumvirate was never combined in the furtherance of a scientific object.

In pursuance of their plan, a building was erected by public subscriptions. It contained small compartments, the atmosphere of which could be charged with any desired gas. In these rooms the patients were allowed to pass a certain time daily.

The principal results obtained by the use of oxygen are summed up in the following table, from a review of Beddoes's work which was published in the *British Library:*

CASES TREATED.	Cured.	Relieved.	Not benefited.
Obstinate Ulcers	2	2	..
Leprosy (?)	5	..	..
Spasms	3	2	..
Gutta Serena	..	2	3
Chlorosis	5	2	..
Epilepsy	1	..	5
Asthma	10	9	3
Cancer	..	3	..
Dropsy of the Chest	2	1	1
Hypochondria	..	1	..
Dyspepsia	3	1	..
Dropsy	2	1	1
Hydrocephalus	..	1	..
Headache	2	2	..
Poisoning by Opium	1	..	..
Paralysis	2	1	1
Scrofulous Tumors	2	1	..
Deafness	1	..	..
White Swelling	1	..	..
Scorbutus	1	..	..
Venereal	1	..	..
Melancholy	1	1	..
General Debility	1	..	..
Continued Fever	1	..	..
Intermittent Fever	1	..	..
Coldness of Extremities	1	..	..
Total	49	30	14

It will be observed that no cases of phthisis are included in this table. The explanation of this is to be found in the peculiar views entertained by Beddoes as to the relation of oxygen to this disease. Accepting without question the reports of Fourcroy as to the ultimate acceleration of the disease by oxygen, he framed the theory that in phthisis there was a change, either in the constitution of the blood or in the substance of the lung, that favored the absorption of oxygen, which was therefore already present in excess. For this reason he considered oxygen as absolutely contraindicated.

In scorbutus, on the contrary, he supposed that there was a deficiency of oxygen in the system, which he thought should be supplied by artificial means.

The labors of Beddoes did much toward establishing the true position of oxygen as a therapeutic agent. They demonstrated, on the one hand, that the ideas at first entertained as to its curative power were extravagant, and, on the other, that it was an agent capable of producing good effects in many cases not reached by ordinary means. The number and variety of diseases in which the treatment was found beneficial suggest at first sight a certain air of charlatanism, but subsequent observers have corroborated nearly all his statements. Nor, when we consider the physiological relations of oxygen, is it more surprising that its use should be applicable to a large range of cases than that modification of diet should be beneficial in so many diverse diseases.

It is very remarkable that results as satisfactory as those obtained by Beddoes should not have led to a more general adoption of the treatment. But, with the exception of Hill and Thornton, who were contemporaries rather than successors of Beddoes, scarcely any British physician seems to have become interested in the matter, and it was allowed to die out with its original promoters. This was doubtless largely due to the difficulties which then beset the production of the gas, and its transportation to the bedside of the patient. Chemical manipulations were then but little understood, and chemical apparatus was very imperfect. Caoutchouc was unknown, and this simple fact alone would have made that very difficult which is now extremely easy. Indeed, when we consider the part which this substance now plays in the manipulation of the gases, it is not too much to say that its introduction was a necessary preliminary to their general use as remedies.

While Beddoes was carrying on his observations in England, the therapeutic use of oxygen was exciting no little attention in Germany. Numerous experiments and observations were made during the decade preceding the opening of the present century. Prominent among them were those of Girtanner, who, following in the footsteps of Fourcroy, gave arsenic dissolved in nitric acid for a large range of complaints, under the impression that the solution imparted oxygen to the system. He was charmed with the effects of *oxygen* given in this way in *intermittent fever*. In the midst of similar speculations and theories, which seem to have taken the place, for the most part, of observations on the practical use of the gas itself, it is not surprising that little real progress was made in determining the true value of the latter.

At Geneva, however, the use of oxygen fell into more practical hands. The results obtained by Jurine served to encourage others. Odier, then a prominent physician at Geneva, took up the new treatment with great zeal, and the *Society for the Advancement of the Arts and Sciences* caused the founding of an institution similar to that of Beddoes. But, as in the former case, this was short-lived, and with its decline the whole subject of the medicinal use of oxygen sank into oblivion. The frightful epidemic of cholera in Europe, in 1832, brought it again into momentary notice, but, as it failed to answer the expectations of those who employed it, it relapsed into its former obscurity.

It is only within the last fifteen years that any serious attempt has been made to bring this agent again into use. Dr. Riadore, it is true, published some observations upon its use in 1845, recommending it in cases of indigestion, debilitated conditions of the liver and kidneys, nervous affections, asthma, etc. But his cases were not numerous or striking, and failed to arouse the attention of the profession.

In 1857 appeared the first edition of a work on Oxygen, by Dr. S. B. Birch, of London. I have not been able to procure a copy of this edition, and the second, issued in 1868, seems nearly a new work. The writer claims to have presided at the *renaissance* of oxygen, and takes to himself the credit of having instigated all that has been done in the past few years to place its use on a solid foundation. His book consists of a selection of cases, preceded by some general remarks upon the properties and uses of oxygen, its *modus operandi*, etc. His ideas with regard to what he styles the *quasi-nascent* condition of oxygen are very peculiar, and are not borne out by the experience of others. Moreover, his style is singularly obscure, and his book lamentably lacking in practical directions for the administration of the remedy. While constantly insisting upon the necessity of *judiciously* selecting cases for treatment, of *judiciously* administering the gas, and of the *judicious* use of adjuvants, etc., he does not give a single practical rule for determining what is judicious in the premises. The cases which he publishes are very striking, one might almost say, marvellous; and the impression which the work as a whole is calculated to make upon the reader is that, in the hands of the author, oxygen is almost a panacea, while at the same time it would be hopeless for the general practitioner to attempt to grapple with a treatment so intricate, and demanding such peculiar skill.

In sharp contrast with this work is the article on Oxygen in Demarquay's *Essai de Pneumatologie Médicale*, published at Paris in 1866. A little too diffuse, perhaps, it is still plain, simple, and to the point. The author tells his story as of one who has studied the literature of the subject, made some experiments himself, and treated quite a number of cases by the use of oxygen, sometimes successfully, sometimes not. With reference to its use in some diseases, while giving the experience and opinions of others, he states frankly that he has had no experience himself, and does not feel competent to judge of its merits. While it appears to me that some of his experi ments on the physiological effects of the gas are defective, yet, others are novel and extremely valuable. His article is marked by perfect candor and frankness throughout, and is by far the best treatise upon the subject extant.

Dr. Hermann Beigel, in his work on Inhalation, published in London in 1866, presents a few considerations upon the use of oxygen, and cites a number of cases from his practice, in which he has used it with more or less benefit. He invented an apparatus for the production of the gas, according to Fleitmann's process, from the chloride of lime. His treatment of the subject is candid and unpartisan, and his conclusions demand respect.

A new era in the history of oxygen is being inaugurated by the invention of Téssie du Môtay, by means of which the gas can be produced in immense quantities from the atmosphere, and at an insignificant cost. Its possible future in relation to medicine and hygiene can as yet be only dimly discerned. When we shall be able to regulate the proportion of oxygen in the atmosphere of the sick-room as easily as we now regulate the temperature; when closely packed and ill-ventilated tenements can be supplied with this element, the free enjoyment of which is necessary to health; when by its use the contamination of the atmosphere by the furnaces of factories and machine-shops shall be prevented or counteracted, who can tell what will be the sum-total of the result? Yet all this seems now attainable whenever the public shall become sufficiently awake to its importance.

CHAPTER II.

MODES OF PREPARATION AND ADMINISTRATION.

In the preparation of oxygen for medical use, *purity* is, of course, of the very first importance. Undoubtedly many of the effects formerly attributed to oxygen, such as the production of bronchial irritation or inflammation, and even of pneumonitis, were owing to impurities in the gas employed.

The first substance from which oxygen was isolated was the peroxide of mercury, and in all the earlier experiments the gas employed was obtained from this source. It was not long, however, before cases of ptyalism occurring, warned experimenters of the danger of using the oxides of mercury for this purpose.

Chaptal showed, by carefully-conducted experiments, that oxygen so prepared contained an appreciable quantity of the metal. The peroxide of manganese was then substituted, and finally chlorate of potash.

Recently quite a number of processes have been added to the list, so that it now embraces a large range of procedures by which oxygen may be obtained with more or less facility. I will touch briefly upon the more prominent of these, only one having been found by experience to be really adapted to the use of the physician:

1. Decomposition of binoxide of manganese. This is accomplished by heating the oxide to a red heat in an iron retort, or by treating it with sulphuric acid. In the first case a high heat is required, and in the second the acid is disagreeable, and dangerous in general practice. Moreover, the gas contains four or five per cent. of nitrogen from the impurities usually contained in the manganese. If commercial acid is used, it also imparts its impurities to the gas, and among them usually a certain proportion of arsenic. These considerations

have led to the complete abandonment of this method in practice.

2. The decomposition of sulphuric acid, or sulphate of zinc. This process depends upon the decomposition of sulphuric acid by heat into oxygen and sulphurous acid, or that of sulphate of zinc into oxygen, sulphurous acid, and oxide of zinc. It is probable that oxygen could be produced in large quantities in this manner at a very small cost, so that it would be available for industrial purposes; but, for the use of the physician, the complexity and cost of the apparatus required render it an undesirable method.

3. Process of Boussingault. This consists in procuring oxygen from baryta, in utilizing the property which the latter possesses of fixing the oxygen of the air at an elevated temperature, and giving it off again when the temperature is raised still higher. It is difficult to manage, however, and the results are not satisfactory. The apparatus, also, is bulky and expensive.

4. Reaction of sulphuric acid upon bichromate of potash. This reaction results in the production of oxygen and chrome alum. About sixteen per cent. of oxygen is yielded by the bichromate. This yield is too small to render the method desirable, aside from the objections belonging to every process which requires a powerful acid to be placed in unskilled hands.

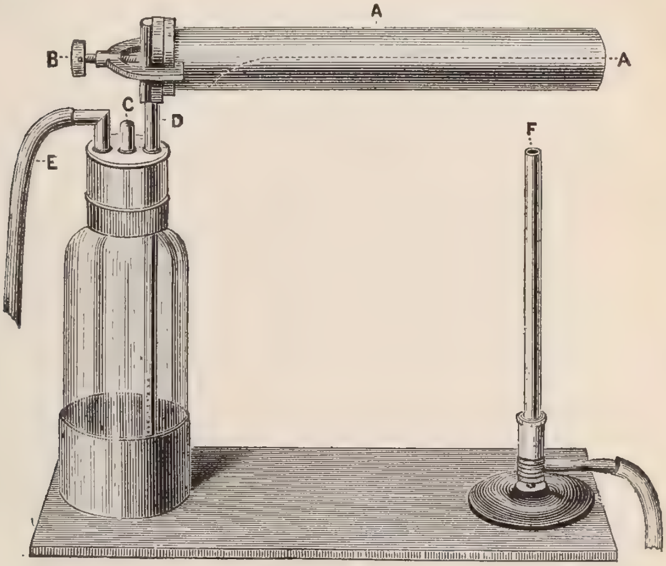
5. Decomposition of chloride of lime by cobalt. The oxide or any of the salts of cobalt have the property of inducing a species of catalytic action between chlorine and lime, from which free oxygen and chloride of calcium result. An extremely minute quantity of cobalt only is required. If a stream of chlorine gas is passed into warm milk of lime, containing a little of a salt of cobalt in solution, the chlorine is absorbed, and oxygen is given off, and at the close of the process chloride of calcium will have taken the place of the lime. This method of procuring oxygen is known as Fleitmann's process. Now, by using chloride of lime, we have the chlorine and the lime united in one substance, and, by merely adding the cobalt, and pouring on a little hot water, the process is greatly simplified. The gas, however, contains

considerable chlorine, and the yield is small in proportion to the quantity of material employed. This process is the one recommended by Dr. Beigel for preparing oxygen for medical use, and his recommendation is sustained by Birch, who, however, prefers compressed oxygen when it can be obtained. I have given the method a trial, but, in my hands, the quantity of gas was so small, and the quality so inferior, that I abandoned its use. However, as the cost is very slight, it might be used with advantage in office practice, where a large stationary apparatus could be employed, and where arrangements could be made for washing the gas through an alkaline solution to remove the chlorine. But, for use at the bedside, an apparatus, small enough to be portable, would not yield the gas in sufficient quantities.

It remains to consider the method of obtaining oxygen by the decomposition of chlorate of potash. This substance, having the formula $KO, ClO^5 = KCl + O^6$, is broken up by heat into oxygen and chloride of potassium.

By mixing with the chlorate a little peroxide of manganese, the disengagement of the oxygen proceeds with greater rapidity and requires much less heat. It is usual to invoke the action of catalysis to explain this, but it seems to me to be owing simply to the facility with which the manganese conducts the heat and diffuses it through the whole mass. Chlorate of potash alone is an exceedingly bad conductor of heat, as is also chloride of potassium. Hence the action of slight degrees of heat is confined to that part only of the mass exposed which is in contact with the retort. But the manganese, being a heavy metallic substance, transmits the heat readily from particle to particle of the salt. Any other substance having an equal conducting power will do as well, provided it will not combine with oxygen. I have succeeded admirably with black oxide of copper. Sand may be used, but with less advantage, as it is comparatively a poor conductor. It is generally stated that this process yields perfectly pure oxygen gas. This, however, is not the case if the evolution is at all rapid, as the gas will then be slightly contaminated with chlorine. There is also another impurity, not noticed in any work on chemistry which I have seen. Pure oxygen, as is well known, is invisible, yet the product from chlorate of potash has usually more or less of a smoky appearance when first evolved. If the gas be allowed to stand for half an hour or an hour, it will lose this appearance, while the glass vessel in which it is contained will be seen to have a deposit on its inner surface. Under the microscope this deposit is found to consist of minute crystalline particles. If enough of these be collected to respond to chemical tests, they will be found to be chlorate of potash. It would seem, then, that a small portion of the chlorate, instead of being decomposed by the heat, is sublimed unchanged, and such is its insolubility that it is not separated from the gas except by repeated washings. Not the least harm results, however, from inhaling it, as I have demonstrated repeatedly in my own person.

The quantity of gas procured by this process is very great, amounting in round numbers to five hundred cubic inches for each ounce of the chlorate of potash, or thirty-nine per cent. by weight. The quantity yielded renders this method peculiarly adapted for use in the sick-room, where portability of apparatus and material is much to be desired. Until recently I have employed a copper flask in which to heat the materials. But I found inconvenience to result from this form of container, inasmuch as the entire quantity of the chlorate was heated at once, resulting in a tumultuous and often unmanageable evolution of the gas. To obviate this difficulty, I have had constructed the apparatus figured in the annexed cut. It consists essentially of a brass retort in the form of a cylinder, nine inches long and one and a quarter inches in diameter, resembling in shape a very large test-tube. To the open extremity of this retort is fitted a cover of cast iron, held in place by a clamp which catches upon a projecting flange surrounding the mouth of the retort. This clamp is tightened by means of a screw. Passing through the cover is the tube carrying the gas into the wash-bottle, and which is arranged at a right angle to the retort. The latter is therefore in a horizontal position, and is supported by its connection with the wash-bottle, which in its turn is firmly fastened to a board forming the base of the whole apparatus. The tube before referred to passes to the bottom of the wash-bottle, and has near its lower extremity a great number of very small holes through which the gas escapes in fine bubbles. This is important, as it insures a much more perfect washing of the gas. Another tube, merely passing through the cap of the wash-bottle, provides for the passage of the gas into the bag from which it is inhaled.



A, retort. The dotted line indicates the manner in which the chlorate of potash is to be disposed. B, clamp, holding the retort in position. C, safety-valve. D, tube leading into the wash-bottle. E, delivery-tube. F, Bunsen burner.

The retort is but half filled with the mixture of chlorate of potash and peroxide of manganese, and this quantity is distributed along its whole length to within an inch of the cover, thus leaving nearly one-half of the diameter of the retort free for the passage of the gas. The heat of a Bunsen burner or of a powerful spirit-lamp is employed, beginning first at the closed extremity of the retort and moving it along as the material becomes exhausted. The wash-bottle is half filled with a solution of caustic potash.

The advantage of this apparatus is, that only a small portion of the material is heated at a time, and the rapidity of evolution is under perfect control. By having two retorts, and using them alternately, a continuous supply may be kept up, sufficient for any emergency. The whole apparatus, including the bag, may be easily packed in a box ten inches square and five inches deep, and a supply of gas may be generated in fifteen minutes, at the house of the patient.

In using black oxide of manganese in connection with chlorate of potash, it is important that it should be free from protoxide, and from any combustible substance. Neglect of this precaution may lead to an explosion.

The process of Téssie du Môtay is as follows: Manganate of soda is exposed to a very high heat in iron retorts, and while in that condition a current of atmospheric air is passed over it. This results in the absorption by the salt of a large portion of the oxygen which the air contains. The current of air is then shut off, and a current of superheated steam substituted. The steam withdraws from the manganate of soda the added quantity of oxygen, and carries it with it to a condenser from which the oxygen passes in a pure state into the gasometer. The salt is then subjected to the action of a second portion of air, followed again by a current of steam, and thus the process goes on indefinitely. The manganate of soda retains its activity, and loses nothing in weight, so that the only consumption is that of fuel.

For use in the sick-room, the gas may be compressed into cylinders of copper or iron, and thus rendered conveniently portable.

In localities sufficiently near to a factory, this is destined to supersede all other methods of supplying oxygen for medical purposes. The gas is perfectly pure, and the quantity which can be compressed into even a small cylinder is sufficient to meet the requirements of any case likely to occur.

The method of administration of oxygen is very simple. The gas, being in a bag or in a gasometer, is conveyed to the mouth or nostrils of the patient by means of a flexible tube, terminating in a mouth-piece of glass, hard rubber, or ivory. This being taken into the mouth, or held to one nostril, the patient breathes the oxygen mingled with a greater or less proportion of common air, one or both nostrils being free for the admission of the latter. The proportion of gas is regulated by the size of the orifice through which it escapes. During *expiration* the rubber tube is compressed between the thumb and index-finger. When the patient is not able to do this for himself, it may be done by an attendant, who, by watching the movements of the chest, soon catches their rhythm. I prefer this plan to the use of an inhaler with a complicated system of valves, which always offers an impediment to respiration.

The quantity given will vary from one or two gallons daily, which is sufficient in some chronic cases, to eighty or one hundred gallons, which may be required in urgent dyspnœa. In chronic cases it should be given from a very small orifice, so that the inhalation of four or five gallons will occupy fifteen to thirty minutes. Feeble patients should take it in the recumbent position.

The inhalations may be repeated morning and evening, or less frequently, as the case may demand. Some very striking results have followed when the interval was as great as three days. On the other hand, when respiration is very much obstructed, it may be necessary to give the gas almost constantly and but little diluted.

Knowing the capacity of the bag employed, and bearing in mind that an adult usually respires from eight to ten pints of air per minute, it is easy to judge approximately of the quantity of oxygen which is being inhaled.

The plan of surcharging the atmosphere of a room with oxygen, and allowing the patient to remain in it for a certain period, has this disadvantage, that, to retain the oxygen, ventilation must be sacrificed. If the room be so large as to do away with this objection, the quantity of oxygen required will be greater than can generally be supplied. These considerations have led to the abandonment of this mode of administration.

Dr. Birch lays great stress upon the gas being given in what he calls a "*quasi-nascent*" condition, that is, he thinks it should be inhaled at once as rapidly as it is generated, or, if not, that it should be kept under pressure until wanted for use. It is enough to say that he brings forward no facts to sustain the alleged advantage of this plan, and that, moreover, others who have not followed it have obtained equally good results.

CHAPTER III.

PHYSIOLOGICAL ACTION OF OXYGEN.

IN regard to the physiological action of oxygen, the first question to be determined is, whether it is possible to cause the blood to take up more oxygen than it receives from the atmospheric air; whether the point of saturation is not attained in ordinary respiration. On this point there was formerly but one opinion. It was thought that there was practically no limit to the power of the blood to absorb oxygen. This idea was no doubt in part based upon the known energy of combustion in pure oxygen gas, and the supposed identity of that process with the retrograde metamorphosis of animal tissue. It was held that the inhalation of pure oxygen would induce rapid chemical action within the body, that a state of general inflammation would ensue, and that destructive metamorphosis would be so much more active than the process of reconstruction that the vital machinery would soon be spoiled, and rendered incapable of continuing its action.

But after a time it was observed that these extreme results did not actually take place, that an animal could remain for a number of hours in pure oxygen without sustaining any apparent injury. This led certain observers to the conclusion that the blood-corpuscle became saturated with oxygen when common air was breathed, and that it would take up no more, no matter how much was presented to it in the air-cells of the lungs. This view was defended by Regnault and Reiset, who endeavored to sustain it by the following experiment: They confined animals in oxygen, and after a time examined the gas, and found that it contained no more carbonic acid than would have been exhaled in the same time if the animals had respired atmospheric air. That these experiments were not conclusive, will become apparent as we proceed.

On the other hand, was cited the fact that animals die in a period varying from three to eighteen hours if confined in an atmosphere of oxygen, and that the tissues present an unusually florid aspect, approaching to a vermilion hue. These observations I believe to be no more conclusive than the others.

On this point Demarquay says: "Science had already taught, what our experiments have confirmed, that animals can live without danger in an atmosphere of pure oxygen, and for a much longer time than in the same volume of air. But beyond a certain limit these animals at last succumb, and one may then satisfy himself that the medium in which they have respired is still capable of relighting an ignited body, a very evident proof that death has taken place from the oxygen itself and not from any alteration which it may have undergone from admixture with the carbonic acid exhaled."[1]

As the test referred to above, that of relighting an extinguished taper, the extremity of the wick being still red hot, is constantly relied upon to prove the respirability of the gas after such experiments, it is well to state at the outset that it is entirely worthless. This is shown by the following experiment:

EXPERIMENT I.—Two parts of pure oxygen were mixed with one part of carbonic acid, prepared by the action of sulphuric acid upon marble. A pint jar was filled with the mixture, which sufficed to relight a taper four times.

Demarquay himself states that ten per cent. of carbonic acid, mixed with oxygen, is sufficient to render the latter incapable of sustaining life; yet we find, by this experiment, that the test which he relies upon would indicate its respirability when containing thirty-three per cent.

The apparatus employed by Demarquay in his experiments, which are essentially similar to those of his predecessors, consisted of a large cylinder furnished with two openings, through one of which the animal was introduced, while to the other a tube was fitted connected with a reservoir of oxygen. The animal, having been placed in the cylinder, a large quantity of oxygen was introduced by the tube, the amount being sufficient to drive out the air in the apparatus, which escaped by the other opening. When it was judged that the cylinder was filled with pure or nearly pure oxygen, both apertures were closed. The animals experimented upon were common fowls, pigeons, and rabbits.

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[1] Essai de Pneumatologie Médicale, p. 644.

The result of these experiments was that, when the animals were allowed to remain in the apparatus for the space of an hour and three-quarters, and were then killed, nearly all the tissues of the body were found reddened to a greater or less extent, but the venous blood retained its darker hue. Two rabbits were allowed to remain until death took place, which, in one instance, was at the end of fourteen hours, and in the other after seventeen hours. At the close of each experiment the gas was found to relight a taper.

These experiments coincide in their results with one of my own, in which the conditions were similar:

EXPERIMENT II.—*June* 10, 1860.—A rat was confined in a jar containing about a gallon of pure oxygen, and inverted over water. At the end of two and a half hours death took place. On opening the body all the internal organs were found to be of a bright-red color.

It will be observed that, in both these instances, no provision was made for removing the carbonic acid and other products of respiration, and that the gas must have become excessively impure. In the following experiments this omission was corrected:

EXPERIMENTS III., IV., and V.—*July* 15, 1869.—A pigeon and two mice were confined respectively three, four, and five hours in jars of oxygen, having a strong solution of caustic potash in the bottom, under a stage upon which the animal rested. The jar was so arranged at the same time that a small stream of oxygen from a rubber bag was constantly flowing into it, and escaping by an aperture of like size. The solution of potash absorbed the carbonic acid, while the gradual change of the atmosphere within the jar was sufficient to prevent a sensible accumulation of other impurities.

These animals when killed did not present any appreciable change in the appearance of the tissues.

EXPERIMENT VI.—*August* 25, 1869.—At 3 P. M. a pigeon was placed in a jar of oxygen, of the capacity of three hundred cubic inches, and the jar inverted over a solution of potash. The following morning the animal was found dead. Upward of one hundred and fifty cubic inches of oxygen had been converted into carbonic acid, and absorbed by the potash, the liquid rising in proportion in the jar. On opening the body *no unusual redness of the tissues was observable.* The feathers of the bird, and also the sides of the jar, were wet with the condensed moisture of the breath. The animal was also in a constrained and uncomfortable position, which, doubtless, hastened its death.

The conclusion which I draw from these experiments is, that the lively red color of the lungs, heart, liver, etc., which are described, and which I have myself seen, does not depend upon hyperoxygenation alone, but also upon a coincident retention of carbonic acid in the tissues. The color pervades the intervascular substance as perfectly as the natural coloring-matter pervades the muscular fibre. It cannot, therefore, be ascribed to simple increase of vascularity.

The following experiments show that, in Demarquay's observations, the oxygen is as little chargeable with the death of the animals as with the change in the color of their tissues :

EXPERIMENT VII.—*August* 13, 1869.—A mouse was confined in a jar of oxygen inverted over a solution of caustic potash. At the end of twenty-five and a half hours, during which he had had neither food nor drink, he was dull and stupid, but, when released, ate greedily, and was soon as lively as ever.

EXPERIMENT VIII.—*August* 16, 1869.—A tin box, seven by ten inches, and six inches deep, open at the bottom, and having the top of glass, was placed in a shallow vessel containing a solution of potash. A little above the surface of the solution was arranged a false bottom of wire-cloth, which formed the floor of the apparatus. A circular opening on one side of the box was fitted with a projecting rim soldered to its edge. To this rim or collar a cap was fitted, and the joint was made air-tight by an india-rubber band stretched around it. This opening was for the purpose of introducing the animal to be experimented upon. A small tube passing into the box was connected with a large reservoir of oxygen. By means of a stop-cock the amount of gas passing into the apparatus was so regulated that a bubble would escape every second or two from under the edge of the box. Within the box was placed an open vessel containing chloride of calcium to absorb the moisture from the breath, and another vessel with dilute nitric acid to take up the ammonia exhaled. Food, water, and a quantity of tow for a bed, having been provided, a mouse was introduced into the apparatus, and the aperture closed air-tight. Oxygen was then admitted freely for some time until all the air was expelled, when the stop-cock was closed to the point already indicated. The animal ate, drank, and arranged his bed, and acted in every particular as mice generally do, until the third day, when he buried himself in the tow, and seemed very quiet. By this time his excretions gave to the gas, which escaped from the apparatus, a very sickening smell.

At the end of four days the mouse was removed and transferred to a cage, where he recovered at once his accustomed liveliness, and appeared no worse for his unique experience.

This single experiment is sufficient to overthrow the theory of hyperoxygenation of the blood, as the term is generally understood, and to show that the fatal results heretofore observed, as well as the peculiar *post-mortem* appearances, were the result of the admixture of the products of respiration with the gas inhaled.

Are we, then, to accept the conclusion of Regnault and Reiset that inhaling pure oxygen makes no difference with respiration? Clinical observation and facts derived from experiments teach us clearly to the contrary.

The quantity of oxygen in the blood under normal conditions is extremely variable. This follows from the varying exigencies of the system. The transition frem perfect repose to active exertion implies increased molecular action and increased consumption of oxygen. The blood-corpuscles are the carriers of oxygen, and, as their number remains the same, each one must assume a greater burden. To explain all the phenomena resulting from the inhalation of oxygen, it is not necessary to assume the absorption of more than corresponds with the extreme limit of this healthy respiratory demand. All the analogies of Nature lead us to suppose that this limit coincides with the point of complete saturation of the blood. To assume a margin beyond it is to suppose a provision against an emergency which can never arise. It is contrary to the economy of Nature that the blood should have the capacity for absorbing more oxygen than Nature can supply.

My view is, then, that if pure oxygen be taken into the lungs, only as much will be absorbed by the blood as would be taken up from the air under circumstances involving the greatest possible physiological demand for oxygen. I know that it has been asserted that blood agitated in a vessel with oxygen will assume a livelier red than when agitated with common air. This, however, is a mistake. The change will take place more promptly with oxygen, but the hue will be in the end the same. We may therefore assume that, if the blood and the air be brought into sufficiently intimate contact in the lungs, the corpuscles will become saturated with oxygen from the ordinary atmosphere.

EXPERIMENT IX.—*August* 20, 1869.—A quantity of defibrinated sheep's blood was divided into two portions. One portion was thoroughly agitated with oxygen, and quickly assumed a bright-red hue. The other was agitated in the same way with common air. The change took place more gradually, but eventually, when the two jars were placed side by side, no difference in the color could be distinguished.

The portion agitated with air was then placed in a vessel filled with oxygen, which was closed tightly, while its interior was made to communicate with a delicate manometer. After the lapse of an hour, during which the vessel was frequently agitated, no change had taken place in the height of the fluid in the instrument, thus indicating that no additional oxygen had been absorbed.

How, then, is this appearance of superoxygenation of the blood to be accounted for, since it never occurs when atmospheric air is respired? The conditions which obtain while breathing oxygen, without removing the products of respiration, are entirely *sui generis.* They differ from that observed when air is substituted, in that the proportion of carbonic acid may become much greater without destroying life. They differ, also, from the effect produced by confining an animal in a mixture of carbonic acid and oxygen, since in the latter case the change is abrupt, while in the former it is very gradual. The experiments of Count Morrozo, and of Bernard, show what an immense difference, in the effect upon the animal, results from this circumstance. I offer the suggestion, therefore, that the red stain of the tissues is the result of the prolonged action of carbonic acid retained in the blood—life, meanwhile, being kept up and the activity of retrograde metamorphosis sustained by a *maximum absorption of oxygen.*

When a considerable quantity of pure oxygen is inhaled, there is usually a sensation of freedom about the chest, as if respiration were easier. Some persons describe a feeling of warmth beneath the sternum, such as results from inhaling a slightly-stimulating vapor. Sometimes a slight degree of vertigo is produced. Generally there is a tendency of the blood to the surface, and the hands and feet, if previously cold, become warm. In some cases this change of the circulation is accompanied by a prickling sensation. The pulse is sometimes accelerated, but more frequently remains unchanged. In cases of debility it is often reduced in frequency. The temperature is but little changed, if at all. I have sometimes observed a disposition to yawn constantly during the inhalation, and there is generally an inclination afterward to sleep. All these effects are more marked when the gas is inhaled fasting.

In reference to the effect of the inhalation of oxygen upon the amount of carbonic acid formed, and of urea excreted, there has been as yet but little research. The experiments of Regnault and Reiset, upon the first point, have been already referred to. The subject, however, is beset with difficulties, and much caution is required in accepting the results of experiments as to the amount of carbonic acid exhaled when breathing a greater or less proportion of oxygen. Different results will be obtained at different times when breathing the same medium under apparently the same conditions as to diet, stage of digestion, exercise, etc. The slightest bodily exertion, or even mental excitement, will vitiate the experiment. In experiments with animals, eructations of gas from the stomach will sometimes add largely to the percentage of carbonic acid obtained.

My experiments on this point have brought out an (to me) unexpected result, viz., that the inhalation of a considerable quantity of oxygen is followed within a few moments by a temporary *decrease* in the amount of carbonic acid exhaled, as is shown in the following table. The experiments were made upon myself:

	Hour.	Cubic inches of oxygen inhaled.	CO_2 exhaled per minute.
EXPERIMENT XI., July 29, 1869.	4.35 P. M.		21 c. in.
	4.45	600	
	5		19
	5.18		20
EXPERIMENT XII., August 3d.	3.25		22
	4	400	
	4.10		16
EXPERIMENT XIII., August 5th.	12.15		19
	12.25	700	
	12.30		16½
	12.45		18
EXPERIMENT XIV., August 5th.	2		19½
	2.50	400	
	2.55		19½
EXPERIMENT XV., August 5th.	2.55		19½
	3.15	1200	
	3.35		17½
EXPERIMENT XVI., August 12th.	6 A. M. (fasting)		18
	6.15	500	
	6.40		17
	7		17½

EXPERIMENT XVII.—*August* 4, 1869.—A pigeon was placed in a jar containing three hundred cubic inches of oxygen, and the jar inverted over a solution of caustic potash. After twenty minutes the oxygen was removed and replaced by common air. In thirty minutes the volume of air had decreased thirteen cubic inches. The following day the same pigeon was confined again in the same quantity of air for the same period, not having previously inhaled oxygen. The decrease amounted to eighteen cubic inches. There was no evidence that the health of the animal had been injured by the previous experiment.

The manner in which oxygen produces this effect is not easily explained. It is possible that its immediate action may be like that of alcohol, which is known to cause a diminution of the carbonic acid exhaled from the lungs. This is the more probable from the similarity of its other effects, when well marked, to those which alcohol produces.

Notwithstanding this temporary decrease of carbonic acid, I am of the opinion that the ultimate effect of oxygen is to cause its increase. The increase is probably small, and it would doubtless be extremely difficult to demonstrate it conclusively, under normal conditions of activity. Still it seems to me that the result of the following experiments could hardly be attributed to mere accident:

EXPERIMENT XVIII.—*August* 12 *to* 24, 1869.—Three observations were taken daily of the amount of carbonic acid exhaled by myself. In all, twenty observations were made, in seven days, nearly every hour of the day being represented. The average of these observations gave 17.2 cubic inches per minute. During the four following days, eleven similar observations were made—the conditions remaining the same, except that from six to ten gallons of oxygen were inhaled each day in divided doses. The average of these gave 18.9 cubic inches, as the amount of carbonic acid exhaled per minute.

These results, while they coincide with the generally-received opinion as to increased activity in the retrograde metamorphosis as resulting from the use of oxygen, show nevertheless that the increase is confined within narrow limits, and thus confirm the view already expressed, that saturation of the blood-corpuscles with oxygen is quickly attained, is a *strictly physiological condition*, and in no way necessitates the setting up of any morbid action within the system.

To test the effect of oxygen upon the amount of *urea* excreted, I made the following experiment:

Experiment XIX.—*December 8 to 22, 1869.*—The urine for each twenty-four hours was carefully preserved, and the amount of urea estimated according to Haughton's second formula. The result is given in the following table :

DATE.	GALLONS OF OXYGEN INHALED.	UREA IN GRAINS.
December 8	..	641
" 9	8	624
" 10	8	561
" 11	10	472
" 12	6	472
" 13	6	590
" 14	12	550
" 15	14	552
" 16	18	510
" 17	..	542
" 18	..	601
" 19	..	546
" 20	..	556
" 21	..	495

This table shows a rapid decrease in the amount of urea, during the first four days after beginning the inhalations of oxygen. It then increased again, but did not attain to the former figure. With the cessation of the oxygen there is again an increase. The average of the days without oxygen is 563 grains, of those with oxygen 541 grains.

So far as these experiments go, they indicate that oxygen causes a *decrease* in the amount of urea formed. This is surprising, if we are to consider urea as the result of *oxidation* of tissue, as is generally held. More extended observation is required before it would be warrantable to call in question the views so ably enunciated upon this point; but, should it be established that the continued use of oxygen really diminishes the excretion of urea, it would place the latter substance in analogy with the smoke resulting from combustion, a product, it is true, of the combustion, but at the same time a measure of the incompleteness of the process.

The quantity of uric acid in the urine is rapidly diminished by the daily use of oxygen. This fact, suspected by Dr. Golden (*Lancet*, March 10, 1866), has been fully established by Kollmann, of Munich.

In the course of the experiments described above, I observed a very striking diminution of the coloring matter of the urine. At the commencement of the experiment the urine was very high-colored, but within twenty-four hours it became very pale, and remained so for several days after the oxygen was discontinued. This paleness was not owing to an excess of water, as the specific gravity never fell below 1.022, and was usually above 1.025.

CHAPTER IV.

THE USE OF OXYGEN IN DISEASES INVOLVING DEFECTIVE RESPIRATION.

The diseases to which the use of oxygen is applicable fall naturally into two classes: those in which respiration is more or less at fault, and which are for the most part acute; and those in which there is defective nutrition, or excretion, and which are chiefly chronic.

In general terms it may be asserted that any disease which gives rise to dyspnœa will be benefited, at least temporarily, by the use of oxygen, in so far as the dyspnœa is concerned.

The manner in which this is effected is sufficiently obvious. The essence of dyspnœa consisting in a disproportion between the quantity of venous blood brought to the lungs and the amount of oxygen which finds access to the air-cells, it is plain that, by causing the patient to breathe an atmosphere containing more than the usual proportion of oxygen, the dyspnœa may be *pro tanto* relieved.

This is well illustrated by the following experiment:

Experiment XX.—*January* 15, 1860.—Tracheotomy was performed on a rabbit, and a tube provided with a stop-cock was tied in the trachea in such a way that no air could enter the lungs except through the tube. The stop-cock was now turned until symptoms of suffocation were produced; the eyes protruded, the pupils dilated, and the whole body was convulsed. The free end of the tube was then brought into a stream of oxygen, when the symptoms were at once relieved, and, though the breathing was labored, there were no signs of actual distress.

Asthma.—Probably there is no other disease affecting the respiratory organs which so immediately suggests the use of oxygen as does asthma. The dyspnœa is often very severe, and the indications of imperfect aëration of the blood very apparent. Every movement, every gesture of the patient, is a plea for more air. It is not remarkable, therefore, that this should have been one of the first diseases in which oxygen was applied. Beddoes employed it in twenty-two cases, ten of which were cured, nine relieved, and three did not receive benefit.

Demarquay describes two cases, one of which was definitively cured, the other greatly relieved. Birch reports a case of absolute cure.

Dr. Howard Pinkney, of New York, administered it recently in one case with the effect of relieving the paroxysms, and of reducing most decidedly their frequency.

Dr. Beigel, of London, reports three cases, two of them hereditary, in which oxygen was used conjointly with inhalations of liq. potas. arsen. The oxygen in each case relieved the paroxysms, and apparently contributed to remove the tendency. At all events, the paroxysms became less and less severe, and occurred at longer intervals, and finally ceased altogether.

The case of the late Secretary Stanton presents an instance in which oxygen was the means of averting a great deal of suffering. He was subject to severe paroxysms of asthma, which, during his last illness, constituted a serious aggravation of his condition. His medical attendant, Surgeon-General Barnes, of the United States Army, procured from New York a supply of compressed oxygen, of which from three to four cubic feet were inhaled daily. It controlled the paroxysms completely, and, so far as the asthma was concerned, met every indication. In a recent conversation with me; General Barnes expressed himself warmly in favor of the gas in similar cases, and stated that both he and his illustrious patient were entirely satisfied with its effects

I have myself administered oxygen in two cases during the paroxysm, with the effect of causing almost instant relief; but, as it was not continued during the intervals, no permanent benefit was experienced.

It will be observed that three of the cases reported by Beddoes received no benefit whatever. I have, also, had a similar experience. A clergyman, subject for many years to infrequent but prolonged attacks of asthma, which had completely baffled all previous treatment, was seen by me at the onset of a paroxysm. The respiratory movements were very rapid and extremely energetic, and the dyspnœa considerable. Still there was no evidence whatever of imperfect aëration of the blood. The countenance was flushed, but not in the slightest degree dusky. Oxygen was administered very freely, about four gallons every half hour, but with no appreciable relief to the dyspnœa.

Pulmonary Emphysema.—Dr. Beigel reports a case in which the shortness of breath was so great as to prevent the patient walking more than a few steps at a time. By inhalations of oxygen, one gallon every three hours, a prompt amelioration was brought about, and at the end of six weeks the distressing symptoms had nearly disappeared. During this time there was no other treatment than inhalations once or twice a day of solution of common salt, two gr. to the oz. for twelve days, after which "pulverized," pure water was substituted. Subsequent to this time a solution of tannin and morphia was used in connection with the oxygen, and in less than two months from the commencement of the treatment the patient was able to return to his business, though he could not dispense with the oxygen for more than a few days at a time without a return of dyspnœa.

Two months later the unnatural prominence of one side of the chest, which previously existed, had sensibly diminished, the area of tympanitic sounds was much decreased, and the amount of respiration had increased from 2.150 to 3.200 cubic centimetres.—*Beigel on Inhalations*, p. 143.

In this case it is evident that something more was accomplished than mere present relief of the dyspnœa. A radical improvement in the disease itself was effected. This was perhaps due to the diminished force of the respiratory movements, and the consequent lessening of the strain upon the delicate tissues of the air-cells, resulting from the relief to the dyspnœa.

A case is reported by Dr. A. H. Smith in the May number of *New York Medical Journal*, 1869, in which the gas was given, but only for a short time. The dyspnœa was greatly relieved, the countenance lost its livid hue, the pulse fell from 122 to 100, and the respiration from 36 to 20.

The relief continued but a short time after the oxygen was discontinued, but was again experienced when its use was resumed.

Croup.—Dr. Beigel describes a severe case of croup, in which the usual modes of treatment had been exhausted without avail, the respiration being 40 per minute and noisy, the pulse too frequent to be counted, lips livid, and face pale and agitated by convulsive movements. The inhalation of one cubic foot of oxygen was followed by decided improvement, and in the course of two or three hours the child fell asleep, awakening convalescent, and making a prompt recovery.—*Beigel on Inhalations*, p. 105.

Dr. Miquel reports a case with symptoms identical with those above mentioned, which was immediately relieved by the use of oxygen, and ultimately recovered.—*Half-Yearly Compendium*, January, 1869.

I have been called in to give oxygen in several cases of croup, but always in the last stages, when the long-continued dyspnœa had led to mechanical engorgement of the lungs, and to poisoning of the nerve-centres by the circulation of unaërated blood. Under these circumstances, although I have been able to relieve the dyspnœa in a measure, still death has been the result. The termination in such cases is the same when the trachea is opened, even if the respiration is thereby made easy.

It is my firm conviction that oxygen will do in croup all that can be done by tracheotomy, but neither the one nor the other is competent to undo the mischief wrought by severe and protracted dyspnœa. Hence the practical rule in the use of oxygen is the same as that in the use of the knife—*use it early.* There is nothing painful, nothing horrible, nothing dangerous about it. Why, then, should it be resorted to only at the last moment, as if it were a more desperate remedy even than tracheotomy?

Diphtheria.—Dr. Beigel gives the history of two cases in which oxygen was used in connection with inhalations of various atomized solutions. In the first case, that of a child six years old, a single administration of the gas roused the patient from a state of coma; the eyes, before dull, became brighter, and the pulse rose from 60 to 75. After five inhalations the tendency to coma disappeared entirely, the countenance assumed a ruddy color, and the appetite returned. The quantity used at each inhalation was one gallon.

In the second case the benefit was also immediate and decided.—*Beigel on Inhalations*, p. 114.

I have given oxygen in but one case of diphtheria, to which I was called, in accordance with my usual experience, only when the child was in *articulo mortis.* For four hours life was sustained by the gas, which could not be withheld for more than a few minutes at a time without producing suffocation. At the end of that time the parents were informed that the case was hopeless, and it was left for them to continue the gas as long as they saw fit, a supply being prepared beforehand. The inhalation was continued an hour longer, and then abandoned. Death took place within five minutes after the gas was withheld.

In this disease, in addition to the impediment to respiration, we have to contend with a septic condition of the blood, tending greatly to depress the powers of life. I believe, with Dr. Beigel, that this latter condition may be prevented or remedied more surely by inhalations of oxygen than by any other resource we possess, while, at the same time, the danger of death by apnœa is obviated.

For the removal of the local affection the usual remedies may be simultaneously applied.

Pneumonia.—Dr. Golden reports in the *Lancet*, for March 10, 1866, a case of double pneumonia accompanied by great dyspnœa, which resolved in four days under the use of oxygen.

I have seen a case in which a circumscribed pleuro-pneumonia, occurring in the course of chronic pyæmia, was apparently aborted within thirty-six hours by the use of the gas.

Although the existence of acute inflammation, as a rule, precludes the use of oxygen, yet, when respiration is seriously interfered with, the danger from this source outweighs all risk from any possible increase of the inflammation which the use of oxygen may occasion. In a case of double pneumonia, therefore, I should not hesitate to employ it, nor should I allow any case of this disease, which appeared to be tending toward a fatal termination, to proceed without a trial of its effect. The fear formerly entertained, that oxygen would excite pneumonitis by its local action, is refuted by its entire history as a remedy. Especially in the typhoid form of pneumonia, I should expect great benefit from the gas.

Dr. Butler, of New York, reports, in the November number of the *New York Medical Journal*, a very interesting case of chronic pneumonia, in which the deposit was rapidly absorbed under the use of oxygen.

Capillary Bronchitis.—In this affection oxygen is of great value, not only in relieving the dyspnœa, but also in diminishing the formation of mucus in the lungs, which latter is in a great measure the mechanical result of excessive inspiratory effort. For, whenever the inspiratory effort is in excess of the capacity of the air-passages to supply the necessary air, the atmospheric pressure within the chest is necessarily diminished, and turgescence of the blood-vessels of the lungs follows, as certainly as hyperæmia of the skin follows the application of a cupping-glass. Inhalations of oxygen, by satisfying the *besoin de respirer*, remove the necessity for the employment of excessive inspiratory force, and in this way lessen the congestion of the mucous membrane, and diminish the tendency to effusion. Hence it is not only palliative but curative.

This is admirably illustrated in a case reported by Dr. A. H. Smith, in the *Medical Record* for June 15, 1869.

The patient, a child, two and half years old, was at the point of death from bronchitis, intercurrent with measles. The respiration was 80, and accompanied by mucous râles audible at some distance from the bed, pulse too frequent to be counted; face pale and dusky, extremities cool. Within one hour after the continuous administration of the gas was resorted to, the pulse had fallen to about 160, and the respiration to 40. Within another hour the face had regained its color, *and the râles were no longer audible, unless the ear were applied to the chest.* The inhalation was continued without interruption for three hours, and irregularly for five hours longer, when it was wholly discontinued. The following day convalescence was fully established.

Dyspnœa from Cardiac Disease.—I have administered oxygen in two cases of dyspnœa from valvular disease. In the first case the patient was speechless, could scarcely be aroused; face livid, no pulse at the wrist, whole surface cold, respiration 40, and extremely labored, pupils widely dilated and fixed. Within fifteen minutes after the administration of the gas was begun, the pulse became perceptible at the wrist, the breathing was easier, intelligence began to return, and waking from his lethargy he complained of cold, and requested more covering. At the end of an hour, the surface was warm and slightly moist, face almost natural in color, lips still blue, pupils almost normal in size and reacting to light. He answered questions intelligently, and swallowed without difficulty. Respiration 25, quite easy. Pulse 120, and of moderate strength, though small. Patient remained quite comfortable for about ten hours, when a sudden paroxysm of dyspnœa proved almost immediately fatal. Attempts were made to give the oxygen, but the extreme jactitation rendered it impossible to do so efficiently.

In the second case the dyspnœa was less severe, but the paroxysms were very much relieved, and often entirely averted, by the use of the gas.

Dr. Const. Paul reports (*Bul. Gén. de Thérap.*, tome lxxv.) a case of dyspnœa (cause not stated), which was relieved by inhalation of oxygen, after the radial pulse had ceased.

Poisoning with Opium.—The effect of opium in reducing the frequency of respiration indicates that the sensibility is so blunted that the blood requires to become unnaturally charged with carbon before the *besoin de respirer* will excite to the act of respiration. Hence the blueness of the face, and the generally asphyxiated appearance. We should suppose *a priori* that oxygen would be useful in such cases, and such has been found to be the fact. M. C. Paul mentions a case in which the gas was used successfully after atropia had failed, and when the patient appeared to be dying. The respiration had fallen to 7, the pulse was very rapid and hardly perceptible, and there was mucous rattling in the throat.—*Bul. Gén. de Thérap.*, tome lxxv.

Poisoning with Charcoal Gas.—M. Paul has been successful in a case in which the face was livid, the surface cold, the pulse very small, and the dyspnœa decided. In such a case the action of the gas is probably due chiefly to its superior displacing power for carbonic acid, in comparison with common air.

Poisoning with Chloroform.—Duroy and Ozanam show, by a number of experiments, that the effect of oxygen is antagonistic to that of chloroform, and they propose that it be used as an antidote.

Birch, on the other hand, asserts that it renders the anæsthesia deeper.

Cholera.—In view of the lividity of the skin, indicating imperfect aëration of the blood during the stage of collapse in cholera, and of the depression of temperature, showing decrease of molecular action, it was natural to hope that inhalations of oxygen would be beneficial, and several physicians have made trial of its use. There is a wide difference of opinion among observers as to the value of the remedy. M. De Smyttere (*Comptes Rendus*, October, 1848) speaks of it in the highest terms. He employed it in the epidemic of 1832, in the algid stage with "full success." He says: "A new animation and a salutary reaction follow promptly the employment of this means, which is entirely rational, and of which no one, so far as I know, has yet thought. I regard the inhalation of oxygen gas during the period of cold and prostration so dangerous in the choleraic attack, and when the intestinal and cutaneous functions are profoundly perverted, as the remedy the most prompt and the most efficacious of those employed up to the present time."

Macrae, in India (1850), and Harvey, in London (1853), also report favorable cases.

On the other hand, in the debate in the Academy, upon the paper of De Smyttere, cited above. M. Hutin stated that he had employed oxygen in numerous cases in 1835, in Africa, without a single case of success.

M. Foy also stated that he had used it in Poland, in 1831, without any especial benefit.

M. St. Ange had had a similar experience, but thought that the difference in the results obtained might be referable to difference in the period in the epidemic, in which the observations had been made.

CHAPTER V.

THE USE OF OXYGEN IN DISEASES INVOLVING DEFECTIVE NUTRITION.

The benefit derived from the use of oxygen, in cases not primarily involving respiration, is to be explained on the principle that it aids defective nutrition. The replacement of old and effete matter by that which is new and active, is fully as much the work of respiration as of digestion. Without the oxygen derived from respiration, *tissue change* would be immediately checked. If the blood does not convey to the tissues the requisite supply of oxygen, the gastro-intestinal system may do its part, and the food may be absorbed, but there the process will be arrested. The material will be on the spot, but the structure will not be repaired. Nor is it enough to present the usual supply of oxygen to the blood in the lungs. The blood itself must be in a proper condition to receive it, or it cannot reach the tissues. The quality of the blood by which it takes up oxygen depends upon the exactness of its chemical constitution. A slight variation in this will affect its absorbing power. But a condition of the blood which prevents the absorption of sufficient oxygen from the diluted medium usually respired, may allow its absorption from a medium in which a greater proportion of oxygen is contained. A deficient absorbing power may be supplemented by an increased supply of the material to be absorbed.

It is on this principle that even a very small quantity of artificial oxygen inhaled each day is capable of producing such decided results in cases appropriate for its use. The deficiency of oxygen in the blood and in the tissues has been very gradually produced, and, once restored, considerable time will be required before the previous condition will again be reached, even if the cause by which it was produced continues operative. Thus, by daily inhalations, the normal condition may be, in a measure, restored and maintained until Nature is enabled to resume her sway.

But there is one form of disease which at the same time depraves the blood and interferes with the function of the lungs, thus striking a double blow at the function of hæmatosis. It may therefore be appropriately considered first, as intermediate between those cases in which respiration is principally involved and those in which nutrition is chiefly at fault.

Phthisis.—Among chronic diseases, pulmonary phthisis was the one which offered from the first the most tempting field for the use of oxygen. It was natural to expect that the profound dyscrasia which lay at the root of the disease might be favorably modified by an agent bearing such intimate physiological relations to the normal blood. At the same time there was room to look for a double local action within the lungs. What might be the result of bringing an excess of free oxygen into direct relation with the tubercular matter in the pulmonary tissue, was a question not less interesting than what might result from the local action of the gas upon the ulcerated surfaces with which it would come into contact.

After the lapse of eighty years these problems still remain but partially solved. With regard to the effect of oxygen upon the system generally in phthisis, we have abundant evidence to show that, *as a rule*, in this disease, as in others in which nutrition is defective, the use of the gas *favors assimilation and results in a gain in weight.* But whether it exerts an influence upon the morbific principle which constitutes the essence of the disease, is a point which facts alone can determine, and up to the present time sufficient facts have not been gathered to warrant a decided answer.

The observations of Hill, Thornton, and others, in the latter part of the past and the early portion of the present century, were made before the days of physical exploration of the chest, and their results are therefore of little real value. They report a number of cases very decidedly benefited, but we cannot be sure that they were instances of genuine phthisis. Since the time of Laennec, we have only isolated cases here and there, in which the gas was given for a few weeks, or at most two or three months, and generally without a very close study of the physical signs. Still, among these cases there is considerable reliable evidence to show that oxygen may sometimes arrest the progress of this disease for a considerable period, and possibly eradicate it altogether. What is required, however, is that a large number of patients in some public hospital should be under careful observation for some time in order to ascertain their real condition and the progress the disease is making, and that the oxygen should then be administered several times a day, and the physical signs watched by a competent observer. Until this is done, neither the general effect upon the dyscrasia nor the local effect upon the lung can be satisfactorily appreciated. Theoretically there is much to encourage to such a trial. First, we have the fact that in phthisis the nutrition is faulty to a degree that has given the disease its name, and the correlative fact that oxygen promotes nutrition in a remarkable manner. Secondly, we know that an impure atmosphere promotes phthisis, and that persons whose occupation keeps them constantly in the open air rarely become phthisical. Thirdly, modern chemistry teaches us that oxidation is the first step in the metamorphosis of tissue which precedes its resorption, as it is the first step in the decay and disintegration of dead organic matter. It is reasonable, therefore, to suppose that the presence of free oxygen in contact with tubercle would initiate a process of disintegration which would favor its absorption. How far the few clinical facts on record bear out this reasoning will be seen hereafter.

Lastly, numerous experiments show that oxygen in contact with a wound or ulcer acts as a stimulant, promoting the formation of granulations, and, if carried too far, setting up active inflammation. We have in this fact at once a therapeutical indication, and also a possible solution of the want of success which has so often attended the use of oxygen in phthisis, especially in the advanced stages. As a solution of nitrate of silver of appropriate strength, applied with discrimination, facilitates the healing of an external ulcer, so oxygen, properly diluted and carefully employed, may have a healing effect upon ulcers of the lung. But as a strong caustic applied indiscriminately to external ulcers would often induce excessive action, so oxygen in excessive proportion may excite inflammatory action in ulcers of the pulmonary tissue, which, reacting upon the general system, would produce fever and other symptoms apparently indicating an aggravation of the general disease. These considerations show how far we are from a knowledge of the possible usefulness of oxygen in this dread disease. The effects heretofore obtained have resulted from a hap-hazard use, such as in the case of any other remedy would have insured its total failure. Only when we learn to use it with the same discrimination and care that we exercise in the use of opium or strychnia, shall we know the limit of its power for good. The quantity and the mode of administration should be regulated by the actual condition of the lung. In the first stage when we may assume that the mucous surface is entire, the gas may be given with impunity diluted with two or three times its bulk of air, but even then the symptoms should be carefully watched, and especially the temperature, and any thing indicating the occurrence of local inflammation should be the signal for further dilution of the gas. There is no necessity for adding to the inspired air more than three or four per cent. of oxygen in order to produce a decided effect upon nutrition by one or two inhalations daily of half an hour's duration each. An atmosphere of this kind could have no appreciable local effect even upon an ulcerated surface.

When there is profuse purulent expectoration, or when the signs indicate the existence of cavities, the local effect of oxygen but little diluted might be cautiously tried. In such cases the expectoration will sometimes rapidly diminish. This is notably the case in chronic bronchitis, and it is probable that some of the favorable cases reported by early observers were instances of this disease rather than of phthisis. But there are cases on record in which oxygen has been for a time beneficial, but after a few weeks has produced the symptoms of local inflammation. It is probable that the action in these cases was the same as that observed when ulcers on the surface have been exposed to the direct contact of oxygen. For a time the ulcer improves, the indolent character is lost, granulations spring up, and cicatrization commences. But soon the action becomes excessive, and inflammation results. By diminishing the action in time, this effect would be avoided and the benefit already procured would be retained. A like management in the cases of phthisis referred to would probably have obviated the difficulty experienced.

The use of oxygen in phthisis need not exclude any of the usual remedies employed. On the contrary, such of them as undergo a process of digestion and assimilation, as, for example, cod-liver oil, would doubtless be more efficacious for being associated with the gas.

I have been able to collect the histories of a considerable number of cases of phthisis in which the use of oxygen was beneficial. I will mention the leading points of some of these, referring the reader to the original source for details, which would occupy too much space in this essay:

Case I.—Reported by Demarquay (p. 733). X——, aged thirty-two. Tubercles in both lungs; cavity in left of the size of an egg. Greatly emaciated, pale, anæmic, profuse expectoration, intense fever in the afternoon, diarrhœa. Tubercular epididymitis. *March 1st.*—To take four litres of oxygen in ten of air, daily. *March 3d.*—The cough is less frequent, expectoration less abundant; slept well. Increased the dose to twelve litres. *4th.*—A little appetite, but little cough, no expectoration. *6th.*—Great appetite, sleeps well, physiognomy better. *8th.*—Increased to fifteen litres. *10th.*—Patient has been up and about for the last two days. Appetite so great that, after eating the dinner provided by the institution, he goes out and dines again in the city. Face has more color, cheeks filling out. Respiration easier. *16th.*—Cavity still remains, but the surrounding tissue which was hepatized now performs its function. *19th.*—Able to take a long walk. *26th.*—Cough and expectoration have entirely disappeared. No gurgling as formerly, respiration still amphoric.

April 30th.—Discharged in a very satisfactory condition.

Case II.—Demarquay (p. 736). Madame De B., aged twenty-seven. Tubercles in both lungs. Emaciation, frequent cough, profuse expectoration, almost no appetite, abundant night-sweats. The appetite improved and the strength increased, and, on the twenty-seventh day of the treatment by oxygen, patient was able to give a dinner-party and preside for two hours at the table. The menses, which had been absent for five months, returned. The cough and expectoration, though less, persisted during the summer and toward autumn; the use of the gas having been for some time suspended, a relapse took place, and death followed the ensuing February.

It is to be remarked that in this case all other treatment was suspended from the moment the use of the oxygen began.

Case III.—Reported by M. Monod (quoted by Demarquay, p. 739). M. C. B., aged twenty-six. For some years has had very abundant hæmorrhages. Signs indicate a number of cavities and an abundant deposit of tubercle, especially in right lung. At the commencement of treatment was so feeble that he could only be moved from the bed to the sofa. Abundant muco-purulent expectoration, complete anorexia. Twelve litres of oxygen were given twice a day, and within a brief period he had so far improved that he was able to walk in the garden, and even to attend occasionally to business. For sixteen months the treatment has been continued, during which time there has been no return of hæmorrhage, the expectoration is now insignificant, and the cough infrequent. The appetite is habitually good. Still the disease is making progress, the pulse is frequent, and the strength less than last year. But the oxygen has restrained the march of the disease, which last spring seemed to have arrived at its last stage. The improvement followed so immediately upon the administration of the gas that it could not be attributed to any other cause.

Demarquay quotes, from notes furnished by M. Hervé de Lavour, an account of that gentleman's experience with oxygen in phthisis which is particularly valuable, as he gives the unfavorable as well as the favorable results. He says: "I have had only nine patients who have been submitted to a somewhat regular course of inhalations of oxygen. Among this number I have obtained three remarkable successes. The first was a phthisical case of old date, having enormous caverns at the summit of one lung, and who, at the time I prescribed the inhalations, had an abundant expectoration, fever in the afternoon, a frequent cough, and such a degree of dyspnœa as rendered the least movement fatiguing. Anorexia was complete. The inhalations of oxygen were prescribed at the dose of fifteen litres" (four gallons) "daily in two portions, mixed with about one-third of common air. Gradually the quantity of gas was raised to forty-five litres daily, taken pure in two doses. Under this influence the expectoration ceased, the appetite returned, and the patient began to convalesce. The gas was continued two months, when the dose was diminished gradually and finally discontinued. For the last five months the patient has gained flesh, follows his usual occupations, and goes on as well as possible."

Case IV.—In the case of the second patient there were crepitant *râles* in almost the entire extent of both lungs, with crackling at both apices, a frequent cough, an abundant expectoration, and a horrible dyspnœa, which compelled her to pass the greater part of her nights in her chair.

"There was a consultation, and two of our most eminent practitioners gave the most unfavorable prognosis. The patient went on from bad to worse, and at last the appetite failed altogether. I prescribed inhalations of oxygen . . . and they were regularly employed for about a month. The dose of the gas was at first about fifteen litres mingled with air, then twenty, and at last forty-five litres were given without any mixture. Under this influence the amelioration was rapid, the dyspnœa disappeared, the appetite returned, and the patient is now as well as possible, attending to all his affairs, and saying that he has never been better."

CASE V.—The third case is a patient having tubercles at the summits of both lungs, and having besides hypertrophy of the heart, with violent palpitations, and a dyspnœa which would not allow the least exercise. He was submitted for a month to inhalations of oxygen, the cough diminished, the appetite, which was nil, returned, as did also the strength. At the end of three months the patient was able to take long walks without experiencing either dyspnœa or fatigue.

"In the case of the other patients the results were much less satisfactory. Three among them, having cavities, experienced a slight benefit; three others perceived no favorable effect whatever, without my being able to discover in the condition of the patients the difference in the results obtained."

I have quoted these cases in full, on account of the clearness with which they are described and the candor with which the writer relates his want of success in the larger number of his cases.

Birch relates two cases, in one of which "auscultation, percussion, and microscopical examination of the expectoration, confirmed the diagnosis as regards tubercular consolidation and central cavity. . . . From the very first dose of oxygen a diminution in the sensations of irritation and weakness of the chest could be felt by the patient; within a month marked improvement evidenced itself both in the lung and general health, and at the termination of four or five months' steady treatment . . . the flattening" (of the chest) "had given way to almost perfect symmetry. . . . The following winter there was still some pain and sensitiveness to the impression of damp and cold air, but otherwise, with a little extra prudence, she enjoyed herself like others. Two years after the commencement of the oxygen she was quite well, married, and has ever since enjoyed good health." (Written eight years after.)—*Birch on Oxygen*, p. 121.

In the second case the upper third of the right lung was full of small cavities, and soon broke up into a large cavity, which, under the influence of oxygen, afterward contracted, causing considerable depression of the wall of the chest. Treatment was continued intermittingly for a year. Patient quite well six years after (p. 123).

During February and March, 1869, a series of experiments were tried at the New York Hospital, on the effect of oxygen in phthisis. Ten men, having phthisis in various stages, were placed in a ward together, and inhaled each about three gallons of oxygen morning and evening. This was continued for about four weeks, when it was found that six had gained in the aggregate forty-nine and a half pounds, while the remaining four had lost seven pounds.

In several of the cases there was a rise in the temperature after the first three or four days, and the gas was discontinued for a short time and again resumed, after which the temperature remained as before the administration. A transcript from the hospital records will be found in the the *New York Medical Journal* for September, 1869, and is worthy of attentive perusal. While the most prominent fact is the increase in weight, yet in several of the cases there was a very marked improvement in other respects, and that, too, when the patient had been growing worse up to the moment of beginning the inhalations.

In those cases in which the gas seemed to disagree with the patient it is probable that the result would have been different if it had been given more diluted.

The figures in Case V. are very remarkable. "February 15th, weight 127 lbs.; March 5th, 124½ lbs.; March 10th, inhalation resumed, weight 123 lbs.; March 16th, 126 lbs." This is one of the cases in which oxygen at first disagreed.

The *Practitioner* for May, 1869, contains an article on the Inhalation of Oxygen, by Edward Mackey, Professor of Materia Medica and Therapeutics, Queen's College, Birmingham. Among other cases are mentioned three of phthisis, all benefited by the use of the gas. All gained in weight, one of them two stone. In two of these cases the disease remained arrested, the third died of pleuritis.

The following cases are from my own notes: Mrs. S., aged twenty-eight, was sent to me by a physician in this city, with the request that I would try the effect of inhalations of oxygen. She had had a distressing cough for three months, expectoration profuse, slight dulness, and tubular breathing at the summit of both lungs, extremely pale and anæmic, utter disgust for food, menstruation had been growing more and more scanty for some months past, and the last two periods had failed entirely. Three weeks before began the use of iron, but was obliged to abandon it as it disagreed with the stomach and caused headache. Six hundred cubic inches of oxygen were inhaled every morning mixed with about four times its bulk of air. For the first week little if any benefit was experienced, after that the appetite began to improve. On the tenth day of the treatment the menses returned and were more abundant and of a better color than for many months previous. The appetite now became very great, the patient declaring that, when returning from the office, she could scarcely wait to reach home, so great was the desire for food. Iron was now borne without difficulty, but after two or three weeks she found that she could not take iron and the oxygen the same day without headache, but could bear either separately. At this time the cough improved rapidly. A simple expectorant mixture had been ordered, but was taken very irregularly. In six weeks from the beginning of the treatment the cough had ceased entirely, the patient had gained flesh and strength and considered herself quite well. The treatment was then discontinued. Five months later she called at my office and reported that since I last saw her she had gained seven pounds, the menstruation had continued regular, and that, in short, she had never been better in her life. Respiration normal at the summit of both lungs.

Mrs. W., aged forty. Had been phthisical for eight years, having frequent hæmorrhages, and being very much reduced in flesh and strength. The summer of 1858 was spent out of town, she being unable to bear the air of the city. While in the country she had a succession of hæmorrhages which left little prospect of even a temporary rally. She improved somewhat, however, and returned to the city, where she passed the winter in a state of extreme feebleness. With the approach of warm weather she prepared to go again into the country, but before doing so decided to try the effect of oxygen, hoping it would take the place of a removal out of town. Accordingly, she inhaled twice a day about four gallons of the gas, and found her strength and appetite so much improved that the change to the country was abandoned. During the summer two very slight hæmorrhages occurred, but her health in the main was infinitely better than the preceding year, notwithstanding the disadvantage of remaining in the city. She has continued the use of the gas, with occasional intermissions, up to the present time. She cannot omit it for more than a week without being sensible of a retrogression. Unfortunately, the physical signs were not noted at the beginning of the treatment, so that we can have no definite measure of her improvement. But the fact remains that the summer, which has always been the most trying season for her, has been passed with great comfort. That this was due to oxygen is shown by the effect of occasionally omitting its use.

On the 7th of December, 1869, I administered oxygen to Miss H., a patient of Dr. Frauenstein, of New York, aged about twenty-five. She was then extremely emaciated, had a distressing cough, and presented, as the doctor informed me, all the signs of pulmonary phthisis. After the first visit she continued the use of the gas under the direction of Dr. F. The 3d of January I received a note from the doctor, stating that the area of dulness in the lung was becoming less, and that although the cough continued she was gaining flesh. He wrote: *She coughs and grows fat.* Three days later she called at my office. The change in her appearance was marvellous. Her previously hollow cheeks had become round and full, and her whole person seemed to have expanded. It was like the change that one sees after recovery from a continued fever. Certainly up to this time nothing could be more gratifying than the progress of this case.

In several other instances I have administered the gas to phthisical patients with considerable benefit, the cough being lessened and the sleep improved, but circumstances have prevented continuing long enough to give decided results.

In other cases, there being no immediate benefit, the patients have become discouraged, and abandoned the treatment without giving it a sufficient trial.

It appears to me that, in the cases which have been mentioned, and which are far from embracing all which have been more or less successful, there is much to encourage the systematic use of oxygen in phthisis. Indeed, considering the extremely limited number of cases in which it has been thoroughly tried, and our ignorance as yet of its proper management, the results may well challenge comparison with those from any other mode of treatment. While not prepared to indorse the opinion of Birch that with the use of oxygen the cure of consumption in its earlier stages should be the rule rather than the exception, I have no hesitation in saying that I have more confidence in it than in any and all other remedies.

Even when it can do no more, it may procure a priceless boon—euthanasia. Often, even after the presence of the shadowy visitor is felt in the sick-chamber, reluctant life maintains a desperate struggle with its adversary, and the gasping sufferer lies for hours pleading for his release, the livid lips, the dilated pupil, and the convulsive breathing, bearing witness to his agony. In such a case the relief afforded by oxygen is beyond all price, as by it the horrible death by suffocation is exchanged for the peaceful falling asleep of exhausted Nature. (See *New York Medical Journal*, May, 1869, p. 149.)

Anæmia.—From what has been already said, it will be seen that oxygen is admirably adapted to this affection. I might select numerous examples to illustrate its effect, but will confine myself to two from my own notes.

Mr. S., aged seventeen, upward of six feet high, has grown very rapidly in the last two years. Eighteen months ago he was attacked with malignant pustule affecting the cheek. He convalesced from this very slowly, and was still feeble when he was seized with articular rheumatism, from which he suffered nearly the whole of last winter. After all inflammatory symptoms had passed, the least effort would bring on excessive pain in the joints, so that he lay for weeks in an almost helpless condition. At last he became able to walk a short distance with the aid of a cane, but still suffered greatly from pain and stiffness in the right hip.

In this condition he first came under my care. He was pale and anæmic, pulse 108, and so feeble that it would be lost every few seconds, and the counting have to be begun anew.

His appetite was good and had been so during his whole sickness. Tongue clean, bowels regular. Had been taking iron and other tonics for a long time with no benefit. He began on the 27th of September to inhale four gallons of oxygen each morning. For the first two weeks the pulse fluctuated between 104 and 120, but by the third or fourth day it had gained decidedly in strength, and could be counted without trouble. His strength improved rapidly; he laid aside his stick, and was able to walk a number of blocks without fatigue. By the 25th of October his pulse had fallen to 84, and was of good volume and strength. The gas was then discontinued. He has now resumed his business of insurance broker, which he conducts with a great deal of energy.

Wilhelm N., aged twenty-eight, had been becoming more and more feeble and bloodless for four months, probably owing to the effects of syphilis, from which he had suffered severely, although there was no longer any external manifestation of the disease. Had been under the care of a physician who gave him quinine and iron, which had benefited him slightly, but, as he was not progressing satisfactorily, he brought him to me for a course of oxygen. I first saw him on the 23d of November. He was then very anæmic, conjunctivæ and nails white, hands cold, pulse 96 and very feeble, no cardiac souffle. Has an absolute disgust for food, and is very restless in his sleep.

Gave him at once four gallons of oxygen, the inhaling of which occupied about ten minutes. To continue the use of tinct. ferri chlor. The following day the pulse had fallen four beats, and there was a little appetite. *25th.*—A further decline of four beats in the pulse, appetite improving. *26th.*—Pulse 84; appetite quite good; complains of constipation and headache; ordered ext. senna fl. *27th.*—Pulse 96; bowels moved freely, but headache still continues. *28th.*—Pulse 80; headache no better. To omit the iron. *29th.*—Pulse 76; head still painful; ordered potas. bromide gr. x ter die. *30th.*—Head somewhat better; pulse 84; appetite very good; nails pink; conjunctivæ still pale, but not so much so as at first. *December 1st.*—Head much better; strength greatly improved. *2d.*—No more headache; has an excellent appetite. From this time he steadily improved.

Dyspepsia.—In this disease oxygen has succeeded when all other means failed. Its most unqualified commendation is by Trousseau, whose knowledge of the resources of therapeutics has rarely, if ever, been surpassed. He speaks of having by its means, in several instances, "*recalled to life* women regarded as lost," from great depression of the digestive powers consequent upon puerperal hæmorrhage or excessive lactation. He cites, as an example, the case of a girl twenty-two years of age recently delivered and exhausted with nursing. Her features were "absolutely those of a *cadaver*." She was admitted into the hospital April 1, 1864. The treatment began by weaning the child, but for fourteen days there was no improvement. She had continuous fever, pulse 120 to 130, skin hot and dry, and constantly-increasing debility. The debility became so great that she could not sit up in bed without fainting, and auscultation was rendered almost impossible. It was ascertained, however, that there were no tubercles. Tonics and iron were tried, but failed completely; the anorexia remained absolute. On the 14th of April, she began inhaling oxygen; very little was given her the first three days; the first attempt to inhale caused syncope. Still by the 19th she was able to sit up in bed with ease and to eat a little. Pulse 104. The 19th, she sat up for an hour, and asked for food. Pulse 92, skin cool. The 24th, the pulse fell to 80; the patient went down into the garden, described her appetite as *voracious.* April 30th, the pulse was 72 to 80, and had been so for the last four days. The patient desired to leave the hospital. But she was still pale, "the living fibre having regained its tonicity sooner than the blood its normal constitution."—*Clinique Médicale de l'Hôtel Dieu*, tome iii., p. 64.

Demarquay also relates similar cases in which the oxygen gave very prompt and decided relief.

Birch insists strongly on the power of oxygen to remove congestions of the liver, and to relieve the dyspeptic symptoms consequent upon them. He cites a number of cases which sustain his views.

With regard to the possible effect of oxygen upon chronic disease of the liver, an observation of Hanfield Jones is very significant. In an article on the "Function and Diseases of the Liver," he says: "The oily contents of the hepatic cells are subject to great variation, both in individuals and in different classes of animals; the less perfect the type of the respiratory process, the greater the quantity of oily matter in the hepatic cells."[1] This statement suggests the inquiry how far fatty liver may be owing to defective hæmatosis. May it not be that the confinement which produces the *foie gras* so delicious to the epicure acts by preventing the reception of a due proportion of oxygen into the blood. Surely the "type of the respiratory process" under such conditions must be any thing but perfect. In the human subject, sedentary habits or improper alimentation may act to diminish the capacity of the blood to carry oxygen, and thus a condition of the respiratory process be produced approximating to the type of those classes of animals in which the hepatic cells are normally loaded with fat. Accepting this hypothesis, the systematic use of oxygen ought to be beneficial in such cases.

Diabetes.—Several cases are recorded in which inhalations of oxygen have caused a temporary disappearance of the sugar in the urine of diabetes, and with it relief from the general symptoms of the disease. Dr. Pinkney, of New York, informs me that he has met with two such cases. Peroxide of hydrogen has been given with the expectation that the second atom of oxygen would be liberated in the system, and result in the more perfect combustion of the sugar. Dr. Richardson has recently employed it in eleven cases, and gives it as his opinion that it is of no value.—*Medical Times and Gazette*, December 12, 1868.

Mr. Byfield, on the contrary, reports (*British Medical Journal*, October 17, 1868) a case *cured* (*?*) in ten weeks.

Demarquay, referring to the effect upon diabetes of a residence in the dense atmosphere of the sea-shore, attributes it to the greater quantity of oxygen inhaled, and adds:

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[1] Braith., January, 1853.

"In support of these facts I may cite the results which I have obtained with inhalations of oxygen in the case of several diabetics, without changing their regimen in the least. I have seen the quantity of sugar in the urine diminish in a notable manner. The figure may descend in a few days to one-half of that usually observed, at the same time that the strength is seen to revive. M. Bérenger-Férand, a very distinguished young naval surgeon, as also Dr. Yvan, who have both applied, at my instance, inhalations of oxygen to the treatment of diabetes, have modified advantageously the condition of their patients. Without doubt, we have only acted upon the symptoms of the disease; but, in the present state of science, what other treatment can we apply to diabetes, since science has not determined either the nature or the real cause of the disease?"[1]

Albuminuria.—In some cases of this disease the albumen in the urine may greatly diminish, or, for a time, disappear entirely under the use of oxygen. I have seen two cases of this kind. The amendment, however, was but temporary. M. Const. Paul reports a similar case.—*Half-Yearly Compendium*, January, 1869.

The frequent occurrence of albuminuria, in pneumonia and other diseases or conditions involving the respiratory function, is a suggestive circumstance in this connection.

Rheumatism and Gout.—But little is known as yet of the effect of oxygen in these diseases. Dr. Golden (*Lancet*, March 10, 1866), observing that the lithic-acid diathesis appeared more decided in cases in which respiration was imperfectly performed, was led to try the effect of oxygen in gout. The result was very satisfactory. Since then Kollmann, an apothecary of Munich, has experimented as to the effect of the inhalation of oxygen upon the quantity of uric acid excreted. He found that it produced an immediate diminution, and that, after continuing it a few days, the uric acid entirely disappeared from the urine. The treatment certainly deserves a trial in these diseases.

Uræmia.—I have not been able to find an instance recorded in which oxygen has been employed in this disease, but it appears to me to offer a chance of benefit. If, as I suspect from experiments already detailed, oxygen has the effect of diminishing the quantity of urea formed, it certainly should be useful in this affection.[1]

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[1] Essai de Pneumatologie Médicale, p. 709.

Headache.—La Passe (*Essai sur la Conservation de la Vie*) states that he cured himself of "*atroces migraines*" by frequent inhalations of oxygen.

Hill cites one case, neuralgic pain in face, which had resisted every form of treatment, and which yielded promptly to oxygen.

Demarquay.—Case of "*migraine*," complicated with neuralgic pains recurring in daily paroxysms, entirely cured by oxygen.

Birch.—Case of "nervo-congestive headache," of long standing, perfectly cured; another case in a lawyer, from overwork of the brain, relieved while taking the first inhalation. A third, connected with uterine derangement, and occurring periodically, entirely cured.

Dr. Mackey, Professor of Materia Medica and Therapeutics, Queen's College, Birmingham.—Three cases of headache, one from chlorosis, one from bilious attacks, and one from depression from over-use of brain and from mental anxiety—all permanently relieved, the headache ceasing each time *during the inhalation* of the gas.—(*Practitioner*, May, 1869.) These are three out of ten cases of various diseases relieved by oxygen. He considers that these ten cases had this in common, that there was venous congestion in some organ or other. Speaking of neuralgia in general, Demarquay says: "The blood is the regulator of the nervous system. According to this idea, it would be right to endeavor to allay certain troubles of the nervous system by introducing into the blood a greater quantity of oxygen. In fact, modifying the conditions of the blood ought naturally to induce a change in the essential character (*manière d'être*) of the nervous system, central or peripheric."

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[1] Since the above was written, Dr. Howard Pinkney has kindly furnished me the notes of a case of uræmia following scarlatina, in a child nine years of age, which was successfully treated with oxygen. There was paralysis on one side of the body coincident with convulsions on the other. The attack had lasted three hours, and the face was dusky and the lips blue. After the inhalation of five gallons of oxygen the color improved, and by the time eight gallons had been taken the lips had a natural vermilion hue, and the paralysis and convulsions had ceased. There was no return, and the patient made a good recovery.

Paralysis.—Beddoes cites two cases cured, one relieved, and one not benefited.

Birch describes a case of complete paraplegia, of "several" years' standing, entirely cured by two years' persistent use of oxygen.

In relation to paralysis, Demarquay observes: "We have seen, in the study of the physiological action of oxygen, that the muscles fix in some sort more especially this agent . . . It is certain that the inhalation of oxygen excites in some persons a necessity for muscular activity. This indication on the one hand, and the anatomical fact which we have cited on the other, prove the powerful effect of oxygen upon muscular action. The fact of paralysis cured under the influence of oxygen comes also to the support of this view."

Epilepsy.—Beddoes claims to have cured one case, while in five other cases he was entirely unsuccessful.

Birch adds to his almost miraculous cures two of epilepsy, one case being of thirty years' standing.

Dr. Ramskill (*Medical Times and Gazette*, July, 1863), reports a case of epilepsy in a young man, aged twenty-three, resulting from syphilis. The attacks occurred every fourteen or sixteen days. He inhaled oxygen twice or three times a day, stopping the inhalation only when a feeling of dizziness came on. At the end of three months he had had only one attack, which was slight. The cachectic aspect which he previously had had disappeared. Two years after he saw him in perfect health. He had had no attack for sixteen months.

Fatty Placenta.—Assuming the correctness of the views upon which Prof. Simpson bases the treatment of such cases with chlorate of potash, we should have in oxygen a more direct and more powerful remedy than any heretofore employed. Once prove that the fœtus perishes from a deficient supply of oxygen through the maternal blood, consequent upon a diseased placenta, and the indication is as plain as in croup or asthma.

Irregularities of Menstruation.—Beddoes and Birch both cite cases in which irregularities of the menstrual function were corrected by the use of oxygen. The first case described by Birch is one in which the return of each menstrual period was attended by a most excruciating headache, beginning two days before the period, and continuing two days after its cessation. During all this time the patient was obliged to remain in bed, in a darkened room. This state of things had continued for a number of years, and had resisted every possible form of treatment. The use of oxygen for a short time produced a perfect cure.

In another case, along with various uterine symptoms, there were fulness and weight about the neck and about the base of the brain, and impairment of nearly all the special senses, and also partial paralysis of one side of the tongue. The patient was at the menopause. A few inhalations of oxygen produced a great improvement in the sight and hearing, and a course of six weeks completed the cure.

CHAPTER VI.

APPLICATIONS OF OXYGEN IN SURGERY.

The effect of oxygen upon open wounds or ulcers is very remarkable, whether the gas be inhaled into the lungs, or applied directly to the part.

The following experiments by Demarquay are of the greatest interest. An incision was made through the skin and cellular tissue, and into the muscles near the axilla, of a dog, and the wound maintained open for three days. At the end of that time the wound was of a grayish color at the edges, and slightly red in the centre. The animal was then placed with its head in a receptacle of oxygen, the opening being made to fit tightly to the neck by means of a band of india-rubber. In two minutes the wound had changed to a lively rose-color. The surface presented small ecchymotic spots, which furnished a slight hæmorrhage. At the same time an abundant serous discharge flowed from the surface of the wound. When the inhalation of the gas was suspended, the wound immediately changed its appearance. The rose-color was succeeded by a grayish hue, and the surface, which had been shining, became dry and dull-looking, resembling a mirror which had just been breathed upon. This experiment was repeated upon several different animals, with similar results.

In the use of oxygen subsequently in cases of great depression of the vital powers after capital operations, M. Demarquay observed, in several instances, the same effect upon the wound.

These observations are of extreme importance. They show the rapidity with which oxygen is taken into the blood, and the effect which it produces upon the reparative processes. They explain also the wonderful power which oxygen possesses, of inducing cicatrization in old and obstinate ulcers, and of which some remarkable instances will be cited hereafter.

Their bearing upon the treatment of the later stages of pulmonary phthisis by oxygen is also very important. That the introduction of pure or nearly pure oxygen into the lungs in such cases should be followed by a certain amount of hæmorrhage from the walls of cavities or the surface of tuberculous ulcers is what they should lead us to expect. They show also that the occurrence of such hæmorrhage should not of itself be considered as contraindicating the use of the gas. On the contrary, it may coincide with the commencement of a reparative process.

Solutions of continuity in the lungs, however, have this peculiarity, that while they partake of the effect of the gas acting through the circulation, they are also exposed to the influence of direct contact. This is of itself capable of producing decided effects, as has been already stated in another connection, and will be hereafter illustrated.

Beddoes, even in the infancy of the use of oxygen, observed its effects upon obstinate and ill-conditioned ulcers. Among others, he quotes a case of scrofulous ulcers of the arms and of one leg, healed in a few weeks under the influence of "vital air."

Another case of ulcer of the leg, which had continued for eighteen years, and had been treated in vain for four years by

Pott, and for seventeen months by Sharp, healed at the end of four weeks under the influence of oxygen. Six months after, there had been no return of the ulcer.

A third case is related of an old scrofulous ulcer on the arm of a debilitated patient. The inhalation of oxygen, in gradually increasing doses, caused an astonishing effect upon the general health, but no tendency to cicatrization of the ulcer. On the contrary, after a time, an inflammatory action was set up. To counteract this, a certain amount of carbonic acid was added, and the inflammation was promptly subdued, and cicatrization took place.

Finally, in a fourth case, there was an ulcer on the leg four inches long and three inches broad, ill-conditioned, and of such a depth as to involve the muscles. After a year of treatment with a vast number of topical applications, together with tonics given internally, there was no improvement whatever. Oxygen was then resorted to, and a very rapid change took place. The general health, which was very much depressed, improved, the strength returned, the ulcer assumed a more favorable aspect, healthy pus was formed, and in fifteen days the sore had closed over three-fourths of its extent. In six weeks from the commencement of the inhalations cicatrization was complete.—(*Considerations on Factitious Airs*, p. 65.) Demarquay describes (p. 792) three cases of phagedenic syphilitic ulcers of alarming extent, which yielded readily to treatment with oxygen. One had existed eighteen months, and had invaded the perinæum, the pubes, and the thighs.

Dr. Golden (*Lancet*, March 10, 1866) reports two cases of fetid phagedenic ulcers, probably of syphilitic origin, which healed very rapidly under treatment with oxygen.

Birch describes a very remarkable cure of a rapidly-spreading syphilitic ulcer of the leg (p. 78). He adds that no other remedy will compare with oxygen as, in common parlance, "a purifier of the blood."

The increase of the recuperative power which oxygen confers has been taken advantage of by Demarquay to enable patients to withstand the shock of severe surgical operations, or the subsequent drain upon the strength which they entail. In debilitated subjects he employs it *before* the operation to prepare them for it, and *subsequently* to enable them to bear up against the after-effects. He is confident that the mortality after operations can in this way be immensely diminished.

In other surgical affections, such as ostitis, caries, etc., especially when associated with anæmia, loss of appetite, and defective assimilation, oxygen is of great value. Demarquay cites a number of cases in point.

Chronic Pyæmia.—My notes present a case of this disease in which oxygen was apparently the means of rescuing the patient from otherwise inevitable death. W., aged fifty years, had been operated upon four weeks previously for stricture of the urethra, by external division. A few days after the operation chills set in, and soon an abscess formed under the pectoral muscles of the right side. Another abscess soon followed on the left arm, and a third over the sacrum. By this time his strength was reduced to the lowest ebb; he was unable to raise his head from the pillow, his pulse was 112, soft and gaseous, countenance Hippocratic; vomited every thing taken into the stomach. Tongue broad, dry, leathery, deeply fissured, of a light-brown color. When I first saw him his friends had already given the necessary directions for forwarding his remains into the country, and a clergyman had administered the last consolations of religion. Within the preceding twenty-four hours a circumscribed pleuro-pneumonia had developed in the right lung. His case, sufficiently desperate before, seemed now perfectly hopeless. Nevertheless, with the consent of the attending physician, I determined to see what could be done by oxygen. About four gallons were administered every two hours, beginning at 11 A. M. Each time the gas was given the color returned to the cheeks and lips, and the patient expressed himself as feeling "brighter." The pulse remained about as frequent, but seemed to gain a little strength. Before night the tongue had become moist at the tip and edges, there was less irritability of the stomach, and the pain in the chest had greatly abated. The gas was given every three hours during the night and the following day. The close of this day found the patient with a tongue moist throughout, pulse 108, no vomiting, no pain, to speak of, in the chest; was able to take freely of stimulants and beef-essence. From this time he gradually but steadily improved. The pleuro-pneumonia disappeared, without running through the usual stages. The appetite returned to a moderate degree. Two more chills occurred, but no further abscesses. The gas was continued about three weeks, the doses being less frequent as the patient improved. At the end of that time convalescence appeared to be established, and the oxygen was discontinued.

Whether in this case the gas exerted any influence upon the septic material in the blood, or whether its effect was merely stimulant and tonic, it is difficult to determine. It is certain, however, that a very remarkable improvement took place simultaneously with the administration of the gas, and which could be attributed to no other agency.

The prolonged contact of oxygen with a denuded surface produces a stimulating effect which varies in degree with the condition of the surface. In the case of recent wounds there is merely a sensation of warmth. In suppurating surfaces the first effect is to render the granulations less florid and softer, and to increase the discharge. If the contact be too prolonged, however, or too frequently repeated, there will be a subsequent reaction so decided as to compel a suspension of the treatment. An analysis of the gas after prolonged contact with a denuded surface shows the presence of an abundance of carbonic acid and a considerable loss of oxygen.—(*Demarquay.*)

It does not appear that the local application of the gas possesses sufficient advantage over its use by inhalation to compensate for the difficulties attending it. Introduced into the blood the action is more uniform and satisfactory, and less liable to become excessive. A transient contact, however, may be of use in cases in which the cutaneous capillaries have lost their tone, and passive hyperæmia is the result. The effect will then be to cause contraction of the vessels, and paleness will take the place of the previous congested hue. Demarquay has illustrated this admirably in a case of eczema rubrum.

CHAPTER VII.

CONCLUDING OBSERVATIONS.

THE foregoing sketch is far from presenting a complete view of the therapeutical results which have been attained by the use of oxygen. Only those points have been brought forward which seem to be sustained by sufficient testimony to entitle them to serious consideration. Numerous isolated cases could be collected in which curative effects have been obtained in a wide range of affections. But enough has been adduced to show that in oxygen we have a remedy capable of aiding us more or less, under circumstances in which the usual resources of therapeutics are inadequate.

Occupying such a peculiar physiological relation to the system, it would be difficult to assign oxygen to any one class in a systematic nomenclature of the materia medica. Of its remote effects the tonic and alterative are the most prominent, while a new word must be coined to express its primary action in dyspnœa. Its local action is stimulant, but is appreciable only in diseased conditions of the surface to which it is applied.

A vast and inviting field for experiment is afforded by the possible effect of oxygen in the whole family of diseases supposed to depend upon a toxic material in the blood. In some of these diseases good has already been accomplished by it. But its action in contagious affections, with the exception of cholera, has not yet been studied, and presents a tempting array of possibilities to stimulate investigation. Especially in New York City, where oxygen will soon be placed within the reach of every practitioner, and in a form entirely free from every thing objectionable or troublesome, it is to be hoped that ere long its relations to disease will be as thoroughly studied as have been already its relations to the economy in health.

There is but one condition which may be considered as contraindicating the use of oxygen, and that is the presence of acute inflammation. But even to this, as we have already seen, there are exceptions. Indeed, it is, after all, a question whether we have any clinical proof whatever of this supposed incompatibility. We do not find in practice that good, pure air aggravates inflammation, nor that the severer forms of the affection select by preference those in whom hæmatosis is most perfect. It may well be, therefore, that this supposed contraindication will prove to be merely a fragment of the exploded idea formerly entertained as to the physiological action of the gas.

It is undeniable, however, that care should be exercised in the use of oxygen in cases in which there are solutions of continuity in the pulmonary mucous membrane. In such cases the local effect of the gas may be developed, and should be vigilantly watched for. The pulse and temperature will furnish the most reliable guides. But, by using the gas very largely diluted, allowing, for example, twenty or thirty minutes for the inhalation of three gallons, any local action will generally be avoided.

In practice, a great difference will be found in different persons in the readiness with which the gas appears to be absorbed, and in the quantity required to produce a given effect. It is possible that this may be owing, at least in part, to the fact observed by Chevreul, that oxygen is more rapidly absorbed when the blood is more than usually alkaline. This suggests the propriety of using alkalies moderately in cases in which the oxygen does not seem to be easily absorbed, provided their use is not contraindicated. It is possible that by their aid oxygen might be used successfully when it would otherwise fail to produce the desired effect.

Art. II.—*Asthenopia, or Weak Sight.*[1] By Edward M. Curtis, M. D., Oswego, N. Y.

The fact that cases of asthenopia are continually occurring in the practice of every physician, and that till recently its manifestations have been so little understood, is my excuse for occupying the time of this Society for a few moments in its discussion. We can all remember how often a patient has come to us whose eyes appeared as clear and bright as one could wish, yet their unhappy possessor would give utterance to a string of bad symptoms so long that we were either much annoyed and considered them as only the sickly fancies of a nervous mind, and sent him away with the sage information that he was growing old; or we became really alarmed, and advised him that it was *probably* an affection of the optic nerve or retina, a commencing glaucoma or amaurosis, and, unless he gave the weakly member rest, some terrible evil, as vague as it was uncertain, would surely overtake him—advice which, though excellent as far as it went, was wholly inadequate to free him from trouble, yet, with the exception of general tonics, when required, this was all that we were able to do, till Helmholtz, Donders, Stellwag, and others, by the aid of the ophthalmoscope and ophthalmometer, made important discoveries in regard to the refraction and accommodation of the human eye.

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[1] This paper was read at the last annual meeting of the Medical Association of Central New York, held at Rochester, N. Y., June, 1869.

Asthenopia may be divided into three varieties—retinal, accommodative, and muscular; and a patient with weak sight may be troubled with one or all of them.

When the reflection of the images of surrounding objects upon the retina causes pain or an uncomfortable sensation, we have the retinal variety, which is at its height in severe cases of photophobia (or intolerance of light); mild examples occur in connection with the accommodative and muscular forms about to be described.

By accommodation, as applied to optics, we understand that change which takes place in the dioptric media (the cornea, aqueous humor, crystalline lens, and vitreous humor) that enables us to see objects at different distances with distinctness. The emmetropic (or normal eye) is so constructed that when at rest only parallel rays of light are brought to a focus upon the retina; a moment's thought will convince one that strictly parallel rays do not exist, as rays of light always diverge from their source, yet the rays, striking the eye, which emanate or are reflected from an object eighteen or more feet distant are so nearly parallel, that, for all practical purposes, they may be called such, and in optics they are considered as having the same relative direction as though they emanated from an object at infinite distances, like the fixed stars. From this we understand that when observing an object more than eighteen feet distant the normal eye is at rest, but if the object be brought nearer to the eye than eighteen feet, those rays from it which pass through the pupil become more divergent, and the nearer the object is brought, the more divergent do those rays become; now, as only parallel rays are brought to a focus on the retina, in order to see a near object distinctly, it becomes necessary that some change should take place in the dioptric media sufficient to deflect these divergent rays in a similar direction to those which are parallel, in order that a clear image may be produced upon the retina. To produce this necessary refraction, the whole eyeball, or some part of its dioptric media, must be so under the control of the will that its convexity may be increased and diminished at pleasure; how this was accomplished has not been understood till recently. The convexity of the whole eyeball was thought to be changed by the action of the external muscles, but, in cases where the recti and oblique muscles are all paralyzed, accommodation may remain unimpaired, as has been proven by a case at Von Graefe's clinique. Helmholtz has shown by measurement with the ophthalmometer that the curvature of the cornea does not change; that the iris is not the active agent is proven by cases of irideremia (or congenital absence of the iris), and also by cases that have occurred in the practice of Graefe and others, where the whole iris has been removed; but it has finally become definitely settled by experiment with the ophthalmometer upon albinos, and cases of irideremia conducted by Çramer and Helmholtz—though entirely independent of each other—that this change of convexity takes place in the *crystalline lens* by the action upon it of the ciliary muscle. This muscle takes its rise from the posterior wall of the canal of Schlemm, forms the connecting link between the choroid and iris in the median tunic of the eye, and is in connection with the lens through the ciliary process and zonula of Zinn. Now, in the accommodative form of asthenopia, this ciliary muscle has become so weakened by overuse or disease, that it is unable to contract sufficiently to keep up the necessary convexity of the lens to enable the patient to view near objects for any length of time.

This is the most frequent form of the disease. Patients complain that, after reading or doing any close work for a few moments, objects become blurred and indistinct; this is on account of the exhaustion of the ciliary muscle; it is too weak to keep up its contraction; it relaxes, the lens becomes less convex, the rays of light from objects observed are brought to a focus behind the retina, on which circles of dispersion are formed, and, as a consequence, the outline of the objects is blurred. If the employment is continued, supra-orbital and temporal pain intervenes, the eyes feel full and watery, the conjunctival and sclerotical vessels become somewhat congested, and dazzling, general headache, dizziness, and even nausea may occur, and the patient is wholly unable to engage at close work for a long time, and may be quite intolerant of light. These are the symptoms of the more aggravated cases; but in practice we meet with every gradation, down to the mere heaviness and blurring of the eyes, that all of us have experienced after having read too long by a bad light, and which is merely an admonition that we are overburdening our ciliary muscles, are straining our accommodation.

In muscular asthenopia, or insufficiency of the internal recti, those muscles are too weak to keep up the necessary convergence requisite to secure perfect binocular vision for near objects. In normally-constructed eyes, there is always formed a separate image of the object perceived on the retina of each eye, and it is necessary that these images should be formed on similar parts of each retina, in order that a sensation of but one object may be conveyed to the brain, otherwise we get diplopia (or double sight). In looking at distant objects, the muscles of normally-constructed eyes are at rest, and images are formed on the same part of each retina; but, to accomplish this, in viewing near objects, it is necessary that the optic axes should be strongly converged, which can only be done by the contraction of the internal recti muscles. Now, if these muscles have become weakened from any cause, their strength is then insufficient to keep up this convergence for a great length of time. They relax; the optic axes become parallel: the images are thrown on dissimilar parts of the two retinæ, and, as a consequence, diplopia takes place. The patient, who, for instance, is reading, does not complain of blurring, but that the letters seem to run into each other. The other subjective symptoms are much the same as in the accommodative form of the disease, but are sometimes more severe; but, by direct examination, the following train of objective symptoms is disclosed: Direct the patient to observe the point of a pencil which you approach toward him in the horizontal and median plane of his eyes. When within about six inches, one—or, if both internal recti are insufficient, each—eye will become uncertain in its fixation, will waver, and finally one of them will deviate outward. The same thing occurs if, after intently regarding a near object for a few moments, a screen be passed before one eye; but this deviation is made still more manifest by holding a prism with its base upward or downward before one eye; this will cause diplopia, and the two images, if the recti muscles are normal, will be one above the other in the same perpendicular plane; but, if there be insufficiency of these muscles, the images will still retain their relative distance one above the other, but will be also separated laterally, and be crossed—that is, the image formed upon the right retina will be seen at the left of the other, and *vice versa*. This lateral deviation may be corrected by another prism placed in front of the first, with its base inward, the two images being brought again into the same perpendicular plane; and the size of the angle of this correcting prism is the true measure in degrees of the amount of the insufficiency of the muscles.

Asthenopia is scarcely ever congenital; but the circumstances which give rise to the disease may be so. The retinal variety, except in the case of some albinos, is always acquired, and is caused by exposure to too strong light, by an inflammation of some of the different structures of the eye, or by the dazzling of indistinct images in the other two forms of the disease. A fruitful cause of the accommodative variety is hypermetropia (a condition of the eye usually dependent on faulty conformation, the antero-posterior diameter of the ball being shorter than natural; as a consequence, parallel rays of light are brought to a focus behind the retina, on which circles of dispersion are formed, and the images of objects are blurred. To compensate for this, the lens is made more convex by the contraction of the ciliary muscle, and in that way parallel rays are properly focused upon the retina). The greater the amount of the hypermetropia—that is, the shorter the antero-posterior diameter of the ball—the greater must be the curvature of the lens and the consequent contraction of the ciliary muscle. This gives clear vision for distant objects; but when the patient attempts to look at near objects, he must call out a still further contraction of this muscle equal in amount to that ordinarily expended by the emmetropic (or normal eye) in accommodation. During youth, while the lens is soft and plastic, this may be done without fatigue; but as the lens hardens by age, the muscle is not sufficiently strong to produce the necessary convexity, and symptoms of asthenopia are soon manifest; and this all the more early if the general muscular system has become weakened from any cause. The accommodation may become weakened to the extent of asthenopia, in the normal, and sometimes even in the myopic eye, by overuse, when the system is suffering from debility, familiar examples of which are the occasional cases of weak sight in women while nursing; patients, while convalescing from typhoid and scarlet fever; students and sewing-girls, who have overworked themselves, and have not been properly nourished; and these cases are always aggravated if there be present ever so little hypermetropia. Cases of astigmatism (that is, where there is a difference in the curvature of the cornea in different meridians) are always asthenopic; and, although primarily due to imperfect refraction of light by the cornea, are, in a measure, dependent on an irritable ciliary muscle. Insufficiency of the internal recti occurs most often in cases of myopia, as this error of refraction necessitates objects being brought so near that a much greater effort at convergence is required than with the normal eye; consequently these muscles become overburdened and enervated. It may also occur in the normal and in the hypermetropic eye, in connection with the accommodative form. According to Knapp and Kugel, cases of asthenopia, dependent upon insufficiency of the external recti muscle, sometimes occur; but they are so infrequent as to have excited little attention.

Stellwag sets it down as an axiom that asthenopia can be cured; better authority we could not wish. In promoting this cure, the first indication—as in other diseases—is to remove the cause. If, in the retinal variety, the photophobia has been caused by iritis, by iridochoroiditis, or by retinitis, the inflammation must, of course, be combated by appropriate treatment. If the patient has been exposed to too strong and brilliant light, that cause must also be removed. If it has occurred from the blurring and dazzling concomitant upon the accommodative and muscular varieties, the means that are successful in removing these latter will relieve the overstimulated retina.

We have always in asthenopia to look to the general health of our patients; and, in the great majority of cases, they will be found anæmic. Tonics are, of course, indicated; and strict hygienic and dietetic rules must be observed. Constitutional treatment alone, however, is not sufficient, if there be an abnormal conformation of the eyes, or if the patient's accommodative or recti muscles have become weakened by overuse; but if, in the case of the overburdened ciliary muscle, we place a weak convex glass before the eye, we relieve the muscle of just so much contractile power as it would take to cause a convexity of the crystalline lens equal in amount to the refraction of the glass used. The strength of the glass will vary with the case; in hypermetropia, the greater the amount of the error of refraction, the stronger must be the glass; and if the hypermetropia be absolute—that is, the ciliary muscle too weak to focus parallel rays upon the retina—glasses must be used for distant as well as for near objects; but in low grades of hypermetropia and for the emmetropic eye, weak convex glasses, for near objects only, are required; and in myopia, where strong concave glasses are used for distance, weak concaves may be necessary for near work.

Insufficiency of the internal recti may also be benefited by the use of weak convex glasses. This is owing to the dependence existing between the act of convergence and that of accommodation; if the latter be relaxed, the tension upon the internal recti is also lessened. These muscles may also be relieved by prismatic glasses, with their bases turned inward, the strength of the prism not to exceed the degree of the insufficiency of the muscles; and they may be strengthened by regular exercise for a short time at stated intervals, with prisms whose bases are turned outward. But, when the insufficiency is extreme, and has been of long duration, division of one or both external recti becomes necessary, after which the proper use of glasses will accomplish a cure. Frequently we find cases where the recti and ciliary muscles are both weakened, and they may require prismatic glasses, one side of which is ground convex—the prisms, to neutralize the insufficiency of the internal recti, and the convexity to relieve the ciliary muscle. But, in practice, it has been found that no one thing conduces so much to the relief of patients suffering from asthenopia as to strengthen and give tone to the weakened muscles by a course of gymnastic exercises taken at regular intervals, as was first proposed by Dyer, of Philadelphia. Having first given the patient those glasses best calculated to relieve the excessive muscular contraction—being careful, however, that they are not too strong—require him to use them at reading or at close work, once in the morning, and once during the afternoon of each day. The first day to read only three or four minutes at a time, and to increase the time by only one minute each consecutive day; and, should the case be very severe, an increase of one minute every other day would be sufficient. When the patient has reached a time above fifteen minutes, he should rest five minutes after every quarter of an hour that he reads, and then proceed until the proper time has been completed; but the exact time should never be exceeded, as in that case there would be danger that the exercise would weaken rather than strengthen the muscles.

Why the diseases of so important an organ as the eye should receive so little attention from the majority of the profession; why they should be considered a sort of Scylla, whose Charybdis, the diseases of the ear, it was only equally necessary to steer clear from; why patients, suffering from them, should be left to the tender mercies of travelling charlatans and patent collyria, is hard to conceive. Certain it is that, since the discovery of the ophthalmoscope, there is no one class of diseases so easily and surely diagnosticated, and about which so certain a prognosis may be indicated, as these same troubles of the eye; and, in regard to asthenopia, though the directions given in this paper may seem dry and hard of application, yet there is not one of us, if he be willing to give a little time to their study, a little practice to their details, but what would find them easily acquired, and would feel amply repaid by the relief and benefit he would confer upon his patients.

Clinical Records from Private Practice.

I.—*Puerperal Convulsions from "Nephritis" successfully treated by Morphia hypodermically.* By R. B. BONTECOU, Troy, N. Y.

JANUARY 26th, 10 A. M., Mrs. L., aged thirty-five years, pregnant seven and a half or eight months, complained of intense frontal headache, pulse 95, general condition feverish, slight anasarca of lower extremities, bowels regular, urine abundant and very light-colored. Anticipating convulsions, I prescribed thirty grains of bromide of potassium every two hours, a cathartic of pulv. jalap comp., and to have cold applied to the head. The urine gave a large albuminous precipitate by the boiling and acid test.

At nine o'clock, P. M., she had a convulsion, and when I saw her twenty minutes afterward was quite unconscious, with heavy breathing, and great congestion of the head and neck; pulse rapid; external os slightly dilated and dilatable; internal os undilatable; movements of child feeble, but no indications of labor. I administered three-fourths of a grain of "morphia sulph." hypodermically; directed hot stimulating injection for the bowels; hot applications to feet and legs, and ice to the head. In rather less than an hour a second very severe convulsion occurred, and I found her twenty minutes afterward profoundly comatose. A consultation was held, and the pulse being still quite frequent, further use of the morphine was advised, and I accordingly used half a grain "morphia sulph." hypodermically. Cold to the head and hot application to the feet to be continued. Found her next morning vomiting severely, probably from the morphine, with which she was yet quite stupid, but could be roused to answer rationally. During one of the efforts at vomiting she cried out with a sharp, sudden pain in the left parietal region, and immediately a queer sensation was felt over the whole right side, accompanied with partial loss of power. Bromide of potassium was occasionally given in thirty-grain doses, and acetate of potassa also in thirty-grain doses; also infusion of apocynum cannabinum. During the five succeeding days she gradually improved, but at no time was her head clear and free from pain, and, although the anasarca was less apparent, the urine became daily more and more albuminous. The child's movements continued feeble since the first day's sickness.

At six o'clock, A. M., sixth day, another severe convulsion occurred, and I found her half an hour afterward quite unconscious, but with no apparent increase of the paralysis. I administered "morphia sulph.," one grain hypodermically, and, although there was no indication of labor, I determined at once to induce it, and save if possible both mother and child from further danger. The patient was placed on her side near the edge of the bed, and a rubber sheet arranged to conduct the water to a tub beneath, and with a syringe and rubber tube, such as is used for injecting subjects, I threw against the os a stream of hot water for thirty minutes, which so relaxed it that I was able to pass into the uterus a rubber speculum plug, and filled it with hot water (had no Barnes's dilator at hand). Chloroform was used by inhalation, during these manipulations, to prevent convulsions. At noon a consultation was again held, and, as there was no labor yet, a larger dilating-bag was introduced, and the woman left until some uterine efforts were provoked, the patient being apparently comfortable and unconscious; directing to continue chloroform sparingly by inhalation whenever there should appear to be any pains.

At five o'clock, P. M., was summoned in haste, and found that a few minutes before she had cried out with one severe pain, but no further convulsions. On examination I discovered that the child and dilator had been expelled, and the child, from being face downward so long, was with difficulty resuscitated. Some ergot was given, placenta soon came away, and the patient remained quiet and comfortable, and made a good recovery. The child was at first feeble (probably a seven-months child), but did well.

This is the second case within a year in which I have used morphine hypodermically with success in convulsions occurring before labor from nephritis, in both instances inducing labor with the douche, and saving both mother and child. Two other cases have come under my observation lately, where convulsions, occurring during and after labor, were also promptly controlled by the use of morphine hypodermically. In publishing the above case, I desire to add my testimony to the efficacy of the treatment in that class of cases.

II.—*Note on Hypodermic Medication, with Cases.*[1] By F. D. Lente, M. D., Cold Spring.

My dear Doctor : The particulars of the case, which I was recently relating to you, and which you considered so interesting as to desire a more permanent record, are briefly these. An officer, a captain of artillery, called me in, one day, during the latter part of the war, while he was just in the acme of a violent chill. He had been sent home from the "Army of the James River" on "sick-leave," for intermittent fever of the obstinate type then prevailing there. He said to me, "I don't care for any thing so much as the terrible headache which succeeds the chill." I told him I thought I could moderate, if not prevent that. I injected about half a grain of morphine dissolved in a little warm water, under the skin. Within ten minutes, as I sat by him, I noticed a gradual subsidence of the shivering; and, within twenty minutes, as well as I can now remember, he was calm, and a feeling of warmth and comparative comfort had pervaded his body, very much to his own astonishment. He had no headache, and very little fever after the aborted chill. This effect of the morphine did not strike me as very extraordinary. I had, several times before, observed the remarkable rapidity with which the injection of this drug would restore warmth to the body chilled from various causes, after the failure of the several devices usually employed for that purpose. In cholera and choleraic attacks, and in the so rapidly-fatal cold stage of " congestive fever," it cannot, in certain cases, be supplemented with success by any other means. In this connection, allow me to notice a remarkable and very suggestive instance of this effect of hypodermic medication, related by Dr. Alonzo Clark, before the New York Academy of Medicine, May 9, 1866, and which you may possibly have forgotten. He found a lady, residing in the family of a medical gentleman, with a cold, moist surface, vomiting, and copious dejections—" restless, nearly pulseless, and apparently dying." Stimulants could neither be borne by the stomach nor by the *rectum.* What resource, then, but the hypodermic method was left? Fifteen drops of Magendie's solution were then injected, " when, after the lapse of some fifteen minutes, the vomiting ceased, the pulse returned, the hands became warm, and a gentle slumber was induced, which lasted for two hours. Then another portion was introduced, which carried the patient through a period of quiet until morning," when she was found out of danger, and only suffering from nausea. This is remarkable, but still more so, when it is further stated that all those alarming phenomena were due to the poisonous effect of an overdose of aconite. What other antidote can equal this?

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[1] This paper, containing some striking instances of the special value of the hypodermic injection of morphia, is forwarded to us for publication by Prof. George T. Elliot, M. D., of this city, to whom the communication was sent in the form of a letter.

I was sent for to visit a distinguished civil engineer a few years ago, residing several miles from here, on the other side of the river, and found him suffering from all the symptoms of sporadic cholera—cold, clammy, and dusky surface, copious vomiting and purging of white fluid, cold tongue, cramped extremities, and husky voice; mind perfectly clear. This gentleman, a man not easily alarmed, had always a special dread of one disease, *cholera.* On his way, on one occasion, from Austria to St. Petersburg, to complete an important contract with the Russian Government, he was informed casually by a friend, whom he met, that there was some cholera there. Without a moment's hesitation, he threw up the contract and retraced his steps. As I could not remain with him long, a prompt and decisive remedy was needed, as also by the urgency and long continuance of the symptoms. What could equal the hypodermic injection of morphia? I tried this to the extent of three-quarters of a grain; and, within half an hour, he was comparatively comfortable, and, strange to say, did not require another dose of any kind. For the debility and derangement succeeding the attack he was sent to Saratoga, to breathe the air, and drink the Congress water, with the desired effect. Allow me, my dear sir, to add one other instance among many, which I might call up from memory, if necessary. I was called, at about the date of the above case, and in the same neighborhood, while riding by, to a poor, white-headed wanderer, some seventy years old or more. He was lying on the ground, in a vacant yard (no one having courage enough to admit him into a house). He was thinly clad, bathed in a profuse perspiration, as cold as death, the wind blowing full upon him, and, by rapid evaporation from the surface, keeping up the chill. He was vomiting and purging the fatal "rice-water." He had soups, and stimulants placed about him by charitable hands, but they were rejected as soon as taken. What chance here for any medication but the hypodermic? Injected a large dose of morphine. He soon felt some relief, which led to the exclamation—"Well, I have been a man-o'-war's-man for over forty years, and I never saw the like of that before." I left him, and after a couple of hours returned, and found him in the same state as at first; regarding it as a hopeless case under the circumstances, I injected about a grain, and left him. Returning the following day, and inquiring after my patient, I learned that he had been laid in a barn, and, in the morning, was well enough to pursue his wanderings. Excuse me, if I tire you with these details, which I never thought of bringing to light; but I am concerned to see that one who, I believe, actually first suggested the use of the hypodermic method publicly in this country (vide *New York Journal of Medicine*, vol. iv., pp. 340, 341), and had the first American instrument manufactured,[1] should be in danger of ignoring to some extent the inestimable advantages of a discovery which may be considered second only to *anæsthesia.*

Yours very truly,
FREDERIC D. LENTE.

COLD SPRING, *October* 15, 1869.

Proceedings of Societies.

MEDICAL SOCIETY OF THE COUNTY OF NEW YORK.

Stated Meeting, February 7, 1870.

DR. GEORGE T. ELLIOT, President, in the Chair.

THE PRESIDENT announced the admission to membership of Drs. Thomas O'Farrell, Leonard Weber, and J. Ghislain Durant. He announced also the death of Dr. Samuel S. Kuypers, a member of the Society since 1820, and of Dr. John T. Tuttle, a member since 1837.

The report of the Committee on Intelligence was read by its Chairman, Dr. Thompson.

OPHTHALMIC THERAPEUTICS.

DR. C. R. AGNEW read a paper with the above title, forcibly presenting those points of ophthalmic practice which should be understood by every general practitioner; contrasting some of the mischievous modes of treatment still too often followed by routine country physicians, with the teachings of science at the present day; touching upon the indications for certain operations often indispensable to the preservation of sight; and giving some practical suggestions from his own experience.

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[1] Dr. Elliot's first trials of the hypodermic method of medication were made in August, 1857.—[ED. N. Y. MED. JOUR.

Mr. Ernest Hart, being called upon by the President, expressed his hearty concurrence in most of the positions taken by Dr. Agnew, and remarked briefly upon a few points.

In the use of astringents great discrimination was required. Properly to treat a superficial inflammation of the eye, it was essential to consider its cause. For instance, the only cases of conjunctivitis in which the bromide of potassium would prove efficacious, were those in which the superficial inflammation of the conjunctiva was a reflex result of retinitis, as in the conjunctivitis of students.

As to the local use of opium, he inclined to think that whatever benefit was derived from it was due to the alcohol of the preparations (e. g., vinum opii.) commonly employed in collyria. To test this point, he had instituted a series of experiments, and found spirit and water, of the same strength as wine of opium, produce just as good effects.

The mode of using nitrate of silver was a matter of some consequence. The popular old method, of dropping a solution into the eye, had but one argument in its favor—that it was the easiest, while it was open to the obvious objection, that the salt thus gets applied where it is not wanted. Children especially should not have inflicted upon them the needless pain caused by such a procedure. In an affection of the palpebral conjunctiva, hold the child firmly, evert the lids, and then either use the diluted stick, or apply your solution with a brush. With the first touch the work of the nitrate is done; and to prevent the pain—by far the most acute—when the lid falls back upon the eyeball, immediately neutralize the superfluous caustic by a solution of common salt, or better, by a little milk, which is less irritating. Similarly, in the use of ointments, do not medicate the whole eye when only a part requires it. A convenient method was to have the ointment made up into small pencils with cocoa-butter.

If there were any exception to the rule that a poultice should never be applied to the eye, it was probably in the case of tinea of the lids, where poultices and glycerine would commonly remove the scabs, which warm water alone would often fail to do.

The application of long-continued cold or of excessive heat might be productive of much harm. A good general rule for the use of warm or cold applications, when there were no indications from the patient's age or diathesis (gouty, rheumatic, etc.), was to treat with warmth inflammations of the cornea, and with cold those of the mucous membranes accompanied by much purulent discharge.

The light should not be excluded from ophthalmic wards, but in those devoted to severe cases it should be modified. In his own wards, the speaker had the window-glass stained blue, and the walls colored green. The blue rays, it was well known, were the ones most readily absorbed by the vitreous humor, and probably few of them reached the retina.

Dr. Knapp spoke of the great value of purgatives in the commencement of strumous ophthalmia. They were also highly beneficial in exudative inflammation of the iris, choroid, or retina, where even a small exudation was liable to produce such serious damage, that we had need of the whole antiphlogistic apparatus—local blood-letting, atropine, etc. A choroiditis, which might destroy the eye in three days, could only be met by the most active treatment. The drain from the bowels was doubtless the best means of producing resorption of exudations, the excessive dejections of cholera sometimes removing the oldest pleuritic effusions. Paracentesis of the cornea, while very useful in a considerable number of cases, was far behind iridectomy for severe serous iritis. Obstinate cases of long standing, with synechia, and showing a tendency to involve the deeper portions of the eye, he thought were rarely, if ever, checked except by iridectomy, though upon this point he considered his experience too limited to warrant a positive conclusion.

Dr. Pomeroy, referring to that part of the paper which had strenuously insisted on the importance of keeping feeble patients in the sunlight, the eyes being protected by a double fold of black silk, related a case of extraction in a weak old man, whom Dr. Agnew had kept all the while directly under a large skylight, with a happy result. He considered the Doctor's method of dressing—the lids kept closed by adhesive plaster, a layer of lint to absorb the secretions, then the black silk, with adhesive strips to confine it—to be the best he had seen. This dressing would not become displaced.

Dr. Prout had proved the efficacy of Teale's method of mercurial inunction—applying cloths spread with the ointment to the inside of the arms. He had thus produced ptyalism in thirty-six hours.

The President extolled the virtues of the quill-toothpick as a dropping-tube for collyria.

A communication was read from the Vice-President, Dr. T. G. Thomas, tendering his resignation, on account of professional engagements, which rendered it impossible for him to be regularly present at the meetings. The Society voted that his resignation be not accepted, and that he be requested to reconsider the matter.

Dr. E. Eliot gave an account of the recent meeting of the State Society.

The report of the Committee on Diseases was presented by its Chairman, Dr. Raborg.

The Society adjourned.

Adjourned Stated Meeting, February 21, 1870.

Dr. George T. Elliot, President, in the Chair.

PATHOLOGY OF ECZEMA.

Dr. Foster Swift read an elaborate paper upon this subject, touching also upon the general principles of treatment.

Dr. Boeck had often found benefit from the administration of iron in the first stage of eczema, the affection being frequently attended by anæmia. In the later stages of obstinate general eczema, he had found a perfectly dry treatment of great value, avoiding baths, which were apt to aggravate the trouble. Of all the ointments, he preferred those made from pitch, of which the less irritant kinds should be selected. General eczema was often accompanied by visceral diseases, for which he always made careful examination. He had found renal disorders in this connection; and the *post mortem* had sometimes revealed cancer of the uterus, unsuspected by the patient during life.

Dr. W. H. Draper had found the paper so exhaustive upon the pathological anatomy of the disease, that the only topics left open for discussion were the etiology and the treatment. As to the first, it seemed to him that we were too much inclined, at the present day, to look upon eczema as a purely local affection, requiring only local treatment. He had been much struck with Dr. Boeck's remark about its frequent association with internal disorders. He thought we often also found it associated with diathetic disorders—for example, the gouty or rheumatic. Its connection with gout, both in acute and chronic cases, he had often observed. In this affection, we had, of course, acting through the capillary circulation of the skin, the same *materies morbi* which we found acting through that of other organs, and producing irritations that manifested themselves by skin-affections well recognized. This view of the etiology was supported by the marked success which would sometimes follow its recognition as a guide to the treatment of cases of eczema, which obstinately refused to yield to local treatment alone. As soon as we began an anti-arthritic regimen, whether medicinal or hygienic, the skin would begin to improve.

Dr. Haight thought that the subject of the diathetic origin of skin-diseases was still very much in the dark, and that little light had been shed upon it by the French apostles of the doctrine. Observations on a large scale showed the percentage of patients suffering from both gout and eczema to be quite small; and, out of three hundred cases of well-marked eczema in the Hospital of Vienna, less than seventy showed any taint of scrofula.

The plan of treatment introduced by Hardy, and employed more extensively by Hebra—that of covering the surface with india-rubber—had been attended with excellent results, and Hebra regarded it as at least an invaluable adjunct. It had the advantage of such simplicity that it could be left in the hands of the patient himself, requiring only cleanliness. It was, moreover, applicable to all stages of the affection, and therein possessed a marked superiority over the complicated treatment of the German school. For example, the tarry applications, so highly commended, he had heard much abused in England, the reason being that they were used at the wrong stage; for tar might be as harmful as helpful in eczema, unless rightly employed.

Of the "impetigo contagiosa" of Dr. Tilbury Fox, he had seen several examples. It was not to be regarded as a true eczema, though it began with a vesicle, almost a bulla, surrounded with scarcely any areola. It was certainly contagious, and it was very amenable to treatment.

Dr. R. W. Taylor had, under Dr. Draper's direction, examined many cases of eczema, with a view to their etiology. He had found abundant in the urine the products of incomplete oxidation—uric acid, the urates, oxalate of lime, etc.; and when oxidizing agents, such as chlorate of potassa and nitric acid, were given, their effect on the skin-disease was remarkable. A dispensary patient, with severe eczema and also gouty symptoms, showed uric acid both in the urine and in the blood. If these cases were carefully examined with reference to their antecedents as well as to their present condition, they would no doubt afford much support to the diathetic doctrine in the modified form held by Dr. Draper, though perhaps not to the extreme views of Bazin and his followers.

Dr. Piffard had at first been unprepared to give any credence to Bazin's theory of the arthritic diathesis; but he had since made careful inquiries of his patients, and found a history of gout or rheumatism in a large proportion of cases, while, in some, concomitant cardiac affections gave further proof of the diathesis. But it was very difficult to decide, from the obscure statements of a patient about his pains, etc., whether or not he had suffered from a gouty or rheumatic tendency. Similarly the connection of eczema with scrofula was apt to be obscured; for commonly there was no marked evidence of scrofula except in the sluggishness of the eruption and of the circulation. In children, the immediate cause was generally found to be some disorder of the digestive organs, whether or not this accounted for the tendency to the disease. In those at the breast, the first signs of extensive eczema were often clearly traceable to a change of diet, and, if suitable breast-milk could be procured, the eruption would very likely disappear of itself. If good milk could not be obtained, cod-liver oil was better for these infants than arsenic or other medicines.

As to the pathological anatomy, the day of the vesicle was evidently past. Milton now claimed that vesicles never occur in eczema, basing his assertion upon the examination of many thousands of cases, in which he had failed to discover one. The speaker had inquired of many dermatologists upon this point, and had not found any who had ever seen an eczematous vesicle. You would often get pseudo-vesicles. From the application of caustic potassa you might get little droplets of serum upon the surface; but touch them with a needle, and the fluid would cling to the needle with no rupture of the epidermis. He had watched these cases with a magnifying-glass, from hour to hour, and had found but one instance of true vesicles accompanying eczema—except in cases of itch, where you might have eczema as a concomitant, and where the vesicles probably belonged to the itch.

Dr. Henry had, in several instances, observed gout and eczema in the same persons. He had also, and especially among the poorer classes, found the strumous diathesis much more frequently associated with eczema than would appear from the tables quoted by Dr. Haight. He wished to give Dr. Swift the credit of having first brought the india-rubber treatment to the notice of the profession in this country.

Dr. Draper would not be understood as advocating the diathetic pathology in a partisan way; but he thought it important to recognize the principle. In two of the diatheses at least—the gouty and the syphilitic—we could hardly fail to do so. The scrofulous diathesis had been mentioned by Dr. Haight as not discoverable in a large number of cases examined by Hebra. Probably no one could, at present, tell exactly what was meant by the scrofulous diathesis, although all recognized it, in an indefinite way, as demanding a certain treatment. With syphilis the case was different. Here we had a poison which, though not yet isolated, we knew to be specific, and which certainly produced some wonderful affections of the skin. So with regard to gout. We knew it was due to the urate of soda, and this we could find deposited in and about the joints. Although the pathological anatomy of eczema had, as before remarked, been exhausted by Dr. Swift, yet we might seek to go a step behind the microscope. The affection was unquestionably a dermatitis, but what produces the dermatitis was the question that concerned us as practitioners. We all recognized certain local causes, and knew the importance of striving to find these in each individual case; but, failing in this, we must search for the constitutional cause.

The President spoke of a lady-patient who had long been wont to cure eczema in her fingers by wearing an india-rubber glove.

Upon motion of Dr. Hammond, a committee of three was appointed to inquire into the merits of a statue of Harvey, by Dr. Horatio Stone, and report upon the expediency of recommending that a bronze copy of the same be placed in Central Park. The President appointed Drs. Hammond, James R. Wood, and Bibbins.

The Society adjourned.

Bibliographical and Literary Notes.

The original of this work[1] was published in Germany in 1868, and is translated under the auspices of Dr. Knapp himself.

It consists of reports of seven cases of glioma of the retina; giving their history, treatment, and the general and microscopical characteristics of the tumors. These reports are followed by a general consideration of the disease in question based on these examples.

In Part II. the same course is pursued in regard to sarcoma of the choroid, of which affection eight cases are reported.

Among the points for diagnosis of glioma are mentioned the age of the patient, the non-occurrence of other forms of tumor in the retina, the ophthalmoscopic appearance (by its golden-yellow lustre, the characteristic ramifications of the retinal vessels, and its diffuseness). For the study of the intraocular appearances in glioma, Dr. Knapp strongly recommends the use of the heliostat. The prognosis of this disease is regarded as very unfavorable. The treatment advised is enucleation of the globe. The same is the case with choroidal sarcoma. It is advisable in both cases to operate *early*, before the disease has spread to the parts external to the globe.

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[1] A Treatise on Intraocular Tumors from Original Clinical Observations and Anatomical Investigations. By H. Knapp, M. D. Translated from the German, by S. Cole, M. D., pp. 323. Wm. Wood & Co. 1869.

The seven cases of glioma of the retina, with eight of choroidal sarcoma, one of "detachment of ciliary body and adjoining choroid from sclerotic," and one of "detachment of hyaloid membrane from retina," comprise the results of Dr. Knapp's personal experience. There is an Appendix of 30–40 pages, containing cases of several other varieties of tumors, taken from the literature on the subject and elsewhere. The book contains one chromo-lithograph, and fifteen other lithographic plates. It is gotten up in good style, all except the translation, which is very defective. The attempt to translate any work *verbatim* into English must always end in very bad grammar.

Dr. Amory's[1] paper on the Bromides is one that possesses great interest just at present, as an aid in estimating the real value of remedies quite the vogue. Aside from this, it possesses the value which is always attached to careful and intelligent experiments. The report of his investigations occupies the "happy mean" as regards length, giving those experiments in detail where any doubt might arise, while the results alone of others are presented.

The article on Veratrum Viride, by Drs. Amory and Webber, published in the *Boston Medical and Surgical Journal*, has been reprinted in pamphlet form. The observations have been made with great accuracy, and the conclusions arrived at coincide in all essential particulars with those of the best foreign observers.

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[1] Experiments upon the "Physiological Action of Bromide of Potassium and Ammonium" (as determined on Man and the Lower Animals). By Robert Amory, M. D. Selected paper read before the Massachusetts Medical Society, June, 1869, pp. 50.

A Contribution to the Physiological Study of Veratrum Viride, and Veratria; with Experiments on Lower Animals, made at La Grange Street Laboratory, 1869. By R. Amory, M. D., and S. G. Webber, M. D. Reprinted from *Boston Medical and Surgical Journal*, pp. 67.

The Medical and Surgical History of the War. From the *Annual Report of the Surgeon-General for* 1869, we learn that one hundred pages of the surgical portion, and six hundred and fifty-seven pages of the medical portion of the first volume of the "Medical and Surgical History of the War" have been printed. The woodcuts, lithographs, and chromo-lithographs for this volume have been completed, the manuscript is in readiness, and the work is progressing as fast as is consistent with the minute accuracy indispensable in statistical matter. The histories of four thousand and sixty-two selected surgical cases of especial interest have been written out for publication in this work.

The *Medical and Surgical Reporter* for January 8, 1870, contains an interesting biographical sketch of Prof. Gross, accompanied by an admirably-executed steel engraving, a full-length portrait of this distinguished member and ornament to our profession.

Announcements.—*J. B. Lippincott & Co., Philadelphia:* Operative Surgery, profusely illustrated. By John H. Packard, M. D. The same house has recently issued "A Manual of Hypodermic Medication." By Prof. Roberts Bartholow, M. D., of Cincinnati.

Recent Medical Works in French.—"Le Mal Française." A translation from Fracastor's work, *de Contagionibus*, 1546, with Notes. By A. Fournier.—"On the Treatment of Deafness and Ringing in the Ears." By J. T. Guérin.—"De la Sciatique." An Historical, Semiological, and Therapeutical Essay. By Dr. P. A. Lagrelette.—"Des Syphilides Vulvaires." By Dr. P. Spillman.

Mr. Le Gros Clark's "Lectures on the Principles of Surgical Diagnosis," which have have been passing through the columns of the *British Medical Journal*, are now collected and issued in very attractive book-form, by Messrs. Churchill, of London. The same house has issued a new work on the "Diseases of Children," by Edward Ellis, M. D.

Dr. Walshe's well-known work on the "Diseases of the Chest" has been translated into French, by Prof. Fonsagrives.

The Professors of the University of Michigan Medical Department announce that they intend to establish a new medical journal, to be called the *Michigan University Medical Journal.* It will be a monthly, and is intended partly as an organ of the Medical Department of the University.

Since the above paragraph was prepared, the first number of this Journal has come to hand. It makes a very creditable appearance, and gives a most excellent selection of papers, with abstracts from other journals, items of news, etc. In their introductory, the editors "distinctly avow that the spirit of the publication is designed to be catholic in the largest sense. Although under the auspices of the University, it is no mere organ of it; nor will it receive attention at the hands of the Journal save only as it may require a fair exponent of its condition and aims before the world." We are pleased to observe and to help in extending this disclaimer; for, with an art so progressive as is the medicine of to-day, and with a professional spirit yearly becoming more and more liberal and advanced, exclusiveism will not meet with a very friendly or liberal support.

Books and Pamphlets Received.—Annual Report of the Surgeon-General of the United States Army, for 1869. Pamphlet, pp. 11.

From this report we learn that, at the date of the last Annual Report, there were forty-nine vacancies in the grade of Assistant-Surgeon; of these fourteen were filled by examination and appointments previous to March 3, 1869, since which date all appointments and promotions in the medical corps have been suspended by the Act of Congress of that date. There are now two vacancies of surgeons, and forty-two of assistant-surgeons in the Medical Corps of the Army.

The number of commissioned medical officers for duty on June 30, 1869, was one hundred and sixty-one; on sick-leave, four; on leave, three. The estimated number of troops in service June 30, 1869, was thirty-two thousand eight hundred and ninety-six; of military posts, two hundred and thirty-nine; besides numerous detachments and temporary outposts requiring a medical officer—an average of one medical officer to two hundred and four men, distributed over the entire country from Alaska to the mouth of the Rio Grande. To supply the deficiency thus created, it has been necessary to employ physicians under contract.

The experience of the past three years has shown that the present organization of the Medical Staff of the Army is the best possible for the interests of the service, and that, even were all the vacancies, now existing, filled, it would be barely adequate in numbers to the demands of our peace establishment. In the British service, where troops are always massed in far larger bodies than in ours, the proportion is one medical officer to one hundred and twenty men, and in both the British and Prussian service the relative rank of the medical officers is greater. In Prussia and Austria the adoption of the organization of the Medical Corps of the United States Army is strongly urged and partially effected. Regimental surgeons and assistant-surgeons are to be done away with, and the entire medical corps will be a staff corps, its officers assigned to their duties, irrespective of regiments, by the Minister of War through the Surgeon-General. So long as our extensive frontiers shall require garrisoned posts for their protection, any reduction of the Medical Corps below the minimum established by the Act of Congress of July 28, 1866, fixing the military peace establishment of the United States, would prove prejudicial to the welfare of the troops, and the efficient discharge of its duties.

Transactions of the Nineteenth Anniversary Meeting of the Illinois State Medical Society, held in Chicago, May 18-20, 1869. 8vo, pp. 160.

Want of space alone compels us to forego the pleasure of a notice of this number of the Transactions, which contains several papers of special interest, and which have already been quite largely copied in many of the medical journals of the country.

The Illustrated Annual Register of Rural Affairs and Cultivator Almanac, for the Year 1870. Albany: Luther Tucker & Son. 1870. 16mo, pp. 128.

Full of interesting information for the farmer or horticulturist.

Vick's Illustrated Catalogue and Floral Guide for 1870. Rochester, N. Y.: James Vick. 8vo, pp. 84.

This catalogue is not only a splendid specimen of work, the engravings being unusually well done, but there are contained in the book instructions and advice on the management and selection of flowers that must prove especially valuable to all who are engaged in the work of horticulture—or even for those whose only garden is comprised within the limits of a few vases set in the window.

Lecture Introductory to the Course on Materia Medica and Hygiene, delivered before the Medical Class of the University of Buffalo, November 12, 1869. By Prof. Charles A. Lee, M. D. Pamphlet, pp. 16.

A powerful plea for scientific accuracy in our therapeutics, and for the systematic and rational study of this important and much-neglected branch of medicine.

First Annual Report of the Children's Hospital, Boston, Mass., with the By-Laws of the Corporation. Pamphlet, pp. 34.

The Pulse of the Insane. By Edward R. Hun, M. D. Pamphlet. Reprint from the *American Journal of Insanity*, for January, 1870, pp. 14. (From the Author.)

Catalogue of the Officers and Students of the University of Michigan, for 1869–'70.

Plastic Operations. A Paper read before the New York Academy of Medicine, February 18, 1869. By Frank H. Hamilton, M. D. This report embraces a brief description of fourteen cases of rhinoplasty, affording examples of the various methods of restoration by transplantation from remote parts—the Italian method (by sliding), the French method (by torsion from the forehead), the Indian or Kooma method.

Seventeenth Annual Report of the Superintendent of the Insane Asylum of California, 1869.

Annual Report of the Mount Sinai Hospital, New York City.

Second Annual Report of the New York Orthopedic Dispensary, 1870.

Tenth Annual Report of the Longview Asylum, Ohio, for the year 1869. Pamphlet, pp. 49.

Forty-fifth Annual Report of Kentucky Eastern Lunatic Asylum, Lexington, Ky., for the year ending September 30, 1869.

Fourteenth Annual Report of the Trustees of the State Lunatic Hospital, at Northampton, Mass., for the year ending October, 1869.

Rules and Regulations of the New York College of Veterinary Surgeons, with a List of the Subscribers.

Reports on the Progress of Medicine.

SURGERY.

1.—*Some Points in Reference to Transverse Fractures of the Patella.* Abstract of a paper by Mr. Jonathan Hutchinson. [Medical Times and Gazette, September 18, 1869.]

The author commenced by stating that, although his conclusions were expressed in a somewhat positive form and without the citation of cases, yet they were not the less based on the careful collation of a large body of clinical evidence. The chief statements were the following: 1. That, after the ordinary transverse fractures of the patella, the upper fragment is not permanently dragged upon by the quadriceps; that, on the contrary, the muscle remains quite passive, and that there is not the slightest benefit from elevation of the limb. 2. That the main cause of separation between the fragments is swelling of the soft parts and effusion into the joint, and that when swelling does not occur, or after it has subsided, it is easy to make the fragments touch. 3. That one of the chief causes of the frequent weakness of the fibrous union which results, is the presence of fluid (synovial) between the fragments, and that it is not usually difficult, by ordinary means, to bring the fragments quite close enough to admit of union, were it not that the presence of fluid hinders its occurrence. 4. That a remarkable weakening of the quadriceps muscle is a common result of these accidents, sometimes amounting to absolute atrophy. That this partial or complete paralysis cannot be explained merely by reference to long rest of the limb, since the flexors do not share it. That it occurs in some cases in which the union is excellent. 5. That in almost all cases the quadriceps becomes slightly but permanently shortened by contraction, so that, however excellent the union may be, the knee cannot be bent without risk of stretching the uniting medium. That the chief danger, after union, consists in allowing the patient to bend his knee, and thus drag the lower fragment downward, there being little or none in allowing him to use the quadriceps as an extensor. 6. That patients with absolute paralysis of the quadriceps are yet able to walk fairly, and suffer no inconvenience whatever from contraction of its antagonists. 7. That "bony union" is probably an exceedingly rare event, while close fibrous union is easy of attainment; that it is quite impossible to distinguish between the two in the living patient, and, further, that all statements as to "bony union" are worthless unless made on examination at least a year after the accident. That the atrophic weakening of the quadriceps explains in many cases the patient's lameness, and that its frequent occurrence tends to reduce the temptation to resort to certain heroic and dangerous methods of keeping the fragments in apposition.

In justice we should state that, in the discussion which followed the reading of Mr. Hutchinson's paper, some of the opinions advanced were not indorsed by the medical men present, nor will these opinions find entire favor with the profession here.

2.—*Coxo-Femoral Resections.* [Chicago Medical Journal, October, 1869.]

M. Good thus tabulates the statistics of 112 coxo-femoral resections performed subsequent to 1860:

Of the 112 cases, 52 (or 46.43 per cent.) were successful and 60 (53.57 per cent.) fatal. In relation to the nationalities, the cases are arranged as follows:

In Germany, 34 operations, 12 (35.29 per cent.) successful.
In England, 32 operations, 21 (95.62 per cent.) successful.
In America, 29 operations, 16 (55.17 per cent.) successful.
In France, 14 operations, and only 2 (those of MM. Sédillot and Backel, at Strasbourg) successful.
In Russia, 3 operations, 1 successful.

Of 52 cures, 19 walked without support, and 9 with a simple cane. M. Good compares with these the results of the expectant treatment at the Hôpital St. Eugénie (Paris), of 12 cases of suppurating coxalgia. One only resulted in spontaneous recovery, three remained stationary during several years, and eight died.—*Gazette des Hôpitaux.*

3.—*The Treatment of Malignant Tumors by Electrolysis.* By W. Neftel, M. D. [Medical Record, September 1, 1869.]

Hon. Th. T. D——, a highly-accomplished gentleman, fifty-eight years old, consulted, last year, several celebrated surgeons in London and Paris (among others, Nélaton), with regard to a tumor in the left mammary region. They all advised him not to undergo any surgical operation, as they considered the tumor a malignant one, the removal of which would only hasten the fatal termination of the undoubtedly constitutional disease. The patient, nevertheless, insisted upon the extirpation of the tumor, and our great surgeon, Dr. Marion Sims, quite successfully performed the operation in Paris. Soon after the cicatrization of the wound, however, the axillary glands of the same (left) side began to enlarge, and, in January last, presented a tumor of the size of an egg, consisting of a conglomeration of enlarged and indurated glands. Dr. Sims again extirpated this second tumor, the microscopical appearance of which was that of a real cancer (carcinoma of the axillary glands). The specimen was presented to the New York Pathological Society, and examined by distinguished histologists.[1] The wound this time healed very slowly, as it was accompanied by dangerous complications, an extensive erysipelas, high fever (107.8° Fahr.), rigors, and delirium. Scarcely had the wound healed, when a new scirrhous tumor began to grow in the right mammary region, and very soon attained the size of an orange, or more. It now became evident that another surgical operation would be useless, for it could only call forth, as before, an immediate relapse, and perhaps in a more dangerous locality. As nothing remained to save the patient, who was perfectly aware of his condition, and whose constitution was broken down, I proposed the electrolytic treatment, expecting, as the best result, merely the local destruction and absorption of the tumor; for, in the present state of our knowledge, I could not have entertained any hope of producing by electrolysis the least favorable change in the constitutional disease.

On the 27th of April, and the 4th and 7th of May, in the presence of Drs. Metcalfe, Nott, and B. Howard, I performed the electrolysis by means of the large apparatus of Krüger and Hirschman, with elements of Siemens, subdividing, at the second and third operation, the cathode into three and four branches, connected with the needles by serres-fines. The latest improvements of the apparatus afforded the possibility of gradually increasing the quantity of the current, without interrupting the circuit, and of diminishing it in the same way, so that the circuit was broken only by the extraction of the last needle. Not a drop of blood escaped. The first operation lasted two minutes, using ten elements; the second five minutes, with twenty elements; and the third ten minutes, with thirty elements. After the operation, the tumor increased considerably in size, but became softer and more elastic. No febrile or other local or constitutional symptoms followed. On the contrary, the patient, who before was weak, anæmic, and cachectic, began to gain strength and flesh; the tumor at the same time diminishing slowly but constantly. A month after the first sitting, the tumor was found a great deal softer and smaller; at the end of the second month it had almost disappeared, and a fortnight later no trace of it remained. The general condition of the patient is now in all respects excellent, and new deposits can nowhere be detected. In his last letter he writes to me as follows: "I am not able to discover any new deposits anywhere, nor would the tumor in the right breast be detected by any ordinary observer. I hope the old devil who took lodgings there, and was ejected, took all his baggage with him."[2]

The above-related case presents the following points of interest:

1. The patient has been examined by a number of celebrated physicians

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[1] Medical Record, March 1, 1869, No. 73, p. 17.

[2] The patient has since returned to the city, and been seen by some of the physicians above named.

in Europe and America, who have all considered him affected by a constitutional cancerous disease; and the extirpated tumors, being real cancers, have proved the correctness of the diagnosis.

2. The described case brings me to the conclusion that the electrolysis must be considered not only as a local agent, as thinks Althaus,[1] but as one capable of modifying, and even curing, the constitutional diathesis. I explain it in the following way: It has already been established, by experimental researches, that the electric current affects powerfully all protoplasmatic structures.[2] Hence it is possible and probable that the cells (which have to be considered as bearers of the contagion, and the cause of the generalization of the disease) get their protoplasma altered in such a way, by electrolysis, as to lose its specific infectious properties, and make it incompatible with the existence and propagation of the cancerous new formation.

3. Finally, this is the first authentic case of cure of a real cancer in a subject affected with constitutional diathesis. I think that if Althaus, to whom we are indebted for the improved electrolytic method, did not succeed in curing a single case of malignant tumor,[3] it is owing only to the imperfection of the apparatus with which he works. I have had one like it imported from London, and have ascertained, by the feeble deflection of the needle of my galvanometer, and by the weak muscular reaction it produces, that Althaus's apparatus generates a very small current-quantity. This explains also why he is obliged to have recourse to so numerous and prolonged sittings (half an hour), while, with the excllent apparatus I am in the habit of using, incomparably better results can be obtained in a much shorter period.

With regard to non-malignant tumors, I will give in my next paper an account of what I have attained by electrolytic treatment. Especially the soft tumors, nævi, etc., yield very rapidly to it. A large goître of eighteen years' standing has completely disappeared in the course of two months. So far as I can judge from my experiments on animals (rabbits), the electrolytic treatment of varicose veins and aneurisms promises to be highly successful. Examining microscopically the thrombi, I could repeatedly convince myself, in opposition to the assertions of Tschaussoff,[4] that the organization of a thrombus really does take place, a fact which had been already experimentelly demonstrated by the classical researches of Virchow,[5] as far back as 1846. Again, it is not difficult to follow up the gradual transformation of the colorless blood-corpuscles into connective-tissue corpuscles, which was likewise accepted by Virchow.

But the most surprising effect can be produced, by the electrolytic treatment, on organic strictures of the urethra. The only case I have had is a gentleman who is yet under my observation. He has been suffering for about ten years from organic strictures impermeable even for the thinnest bougies. He told me that, though he had been under the care of many distinguished surgeons, no one could ever succeed in introducing a catheter into his bladder. On the 20th of July I introduced a French catheter, No. 3, up to the principal stricture, situated in the prostatic part of the urethra, the prostate itself being enormously enlarged, and, by a very simple contrivance, directed the electrolytic action of the negative pole upon the stricture during two minutes. Immediately, to my great astonishment, the catheter passed within the bladder, and an immense quantity of turbid and decomposed urine was discharged. Since this the patient has been able to pass urine easier than he has ever done before. On the 24th July, I repeated the operation with the same result, but using catheter No. 6 of the French scale; and I can now introduce Nos. 8 and 10 without resorting to electrolysis. So far as I can ascertain, the prostate itself does not seem to be enlarged any longer.

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[1] *On the Electrolytic Treatment of Tumors.* London, 1867, p. 10.

[2] Vide KUHNE: *Lehrbuch der physiologischen Chemie*, p. 333.—GOLUBEW: *Wirkung electrischer Schläge auf die farblosen Formbestandtheile des Blutes.* Cent. f. med. Wiss. 1869. No. 5.

[3] Medical Times and Gazette. 1868, p. 469.

[4] Archiv. fur klin. Chirurgie. xi., 184.

[5] Gesammelte Abhandl., p. 323.

In spermatorrhœa this mode of treatment cannot be surpassed. I have had several cases of inveterate spermatorrhœa, which all yielded to a single or to repeated electrolytic treatment of the prostatic part of the urethra. I am sure that those who have once tried this method will find it far superior to all the others, which are comparatively tedious and uncertain.

The first discoverer of the electrolytic treatment was Crussel,[1] of St. Petersburg, Russia. Already in 1839 he demonstrated experimentally the different effects produced by the different poles, and used electrolysis in the treatment of strictures, exudations, tumors, and ulcers. A number of others followed him, among whom one of the most successful is undoubtedly my friend Dr. Moritz Meyer,[2] of Berlin. Dr. Althaus has quite recently improved the method and shown the great importance of the negative pole in the treatment of tumors. Certainly every observer will agree with him that electrolysis, besides annihilating the pain, acts in a threefold manner,[3] viz.: 1. Through mechanical disintegration of the tissues by the nascent hydrogen; 2. Through the dissolving action of the accumulated free alkali (potash, soda, and lime); 3. Through the local modification of nutrition (by means of the vaso-motor nerves) of the parts brought under the immediate influence of the current. To these local effects I can now add, from my own experiments and observations, the constitutional effect of electrolysis, which latter especially makes this method invaluable in many hitherto incurable diseases. One of its great advantages is that it is never followed by inflammation, suppuration, sloughing, or other disturbances, and that the patient can continue his usual occupation and mode of life.

The electrolytic treatment is calculated to open a large field for surgery, and will be applied very soon to a variety of surgical diseases, to the advantage of the profession and the benefit of suffering humanity. The surgeon now, besides his biological knowledge, and the use of his mechanical appliances, will acquire and appropriate to himself the knowledge of physics, electro-physiology, and the management of the complicated galvanic apparatus.

4.—*A Case of Fracture of the Odontoid Process of the Axis, with Discharge of the Bone through an Ulceration into the Pharynx; Recovery.* By W. Bayard, M. D. [Canada Medical Journal, December, 1869.]

I believe the following case to be unique, inasmuch as I am not aware of any record of recovery after fracture and displacement of the odontoid process.

In September, 1864, I was called, with Dr. Botsford, to visit Charlotte Magee, of this city, a little girl of six years of age, well formed and robust, who had been laboring for three weeks under what was supposed to be "neuralgic pains" of the head and neck.

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[1] Crussel: *Die electrolytische Heilmethode.*—Medic. Zeitung Russlands. 1847-48.

[2] Moritz Meyer: *Die Electricitat in ihrer Anwendung auf practische Medicin.* Aufl. 1868, pp. 405, 407.

[3] *Op. cit.*, p. 441.

Her mother reported that, in August, the child fell from a pile of boards, about five feet high, striking on her head and neck; that she cried severely at the time. and could not move the head without pain, but no other indication of injury was observed; that the inability to move the head continued, and the pain in the neck increased to such an extent, that on the sixth day after the accident medical aid was sought.

I found her able to walk well, though she moved carefully, and supported her head with her hand placed under her chin. The head was inclined forward and to the right side, and any attempt to rotate or remove it caused great pain; there was little swelling or pain upon pressure on the occipito-cervical region, and no irregularity could be discovered in the vertebræ of the neck; the pulse was natural, and the general system was undisturbed; beyond the pain and inability to rotate the head, there was nothing to indicate the severe character of the injury, though it was apparent that the child had been badly hurt.

Warm fomentations and chloroform liniment were applied to the neck, and bromide of potash and anodynes given to relieve pain, with directions to keep the child perfectly quiet and as much as possible in the recumbent posture.

I did not see her after that till May (nine months after the accident), when her mother brought her to me. She walked well, though she constantly supported her chin with her hand placed under it. The head rested upon the right shoulder and could not be raised from it without causing severe pain. The neck was much altered in shape, and there was an irregularity in the region of the axis and atlas that gave the idea of a partial luxation of those vertebræ. Her general health was good, and the muscular power was perfect.

Her mother stated that, about two months after my visit, the child was sitting at a table playing, when she suddenly called out, "Oh! mamma, hold my hands;" that, when she got to her, both arms and legs were powerfully thrown back and moving convulsively; that she had not the power to support her head, which fell from side to side. Her mind was not impaired, she conversed freely, and did not complain of pain. She was immediately placed in bed, when the spasmodic action of the limbs subsided; she fell asleep and slept quietly through the night. The next morning she was perfectly powerless from the neck down; she swallowed with difficulty, but articulated well, and the sphincter muscles retained their power. She continued in this state for three months, after which time the power of locomotion very gradually returned.

The history of the case and the appearance of the child rendered it evident that some serious lesion had taken place involving the cervical vertebræ and spinal cord, and, fearing that any sudden movement might produce such pressure upon the cord as to cause instant death, I had an apparatus made by which I could fix the head and *gradually* raise it from its position on the shoulder. The apparatus was made somewhat upon the principle of "Wiess's support for wry-neck," with a bar passing over the head, to which a strap was attached to support the chin, and the head could be moved and placed in any position by means of recompense screws.

She wore the apparatus for nearly a year, when she was able to leave it off with the head nearly erect, the neck tolerably straight, and possessing considerable power of rotation. There is a depression behind the right sterno-mastoid process, and a corresponding elevation upon the opposite side.

She continued in this state, with more or less pain, until March, 1867, when she was brought to me complaining of sore throat; upon examination I found tumefaction and redness upon the posterior part of the fauces over the body of the axis. Suspecting that an abscess was about to form, she was directed to gargle frequently with warm water, and return in a week, at which time she appeared, her mother producing the bone, saying that the child had coughed it up in her presence the day before. I found an opening corresponding in size to the bone, near the body of the axis. In a short time the opening closed, the pain ceased, and the child, to use her mother's words, "got well."

The power of locomotion is perfect; she can walk or run at pleasure, can rotate the head pretty well, and is at present going through a severe ordeal, in the shape of an attack of whooping-cough. I may mention that she has, on several occasions, during a paroxysm, lost all muscular power, and fallen in consequence of pressure upon the spinal cord, but the effect has been momentary.

That the bone in my possession is the odontoid process,[1] I think there cannot be a doubt, and that ulcerative action, sufficient to disengage it from its position, should take place with so little constitutional disturbance, is remarkable. It should teach us to admire the wonderful conservative power of Nature.

5.—*When and when not to trephine.* [Chicago Medical Journal, October, 1869.]

Baron Larry, in his important work on Trephining, in traumatic lesions of the skull (published by Victor Masson & Co., Paris), thus summarizes the indications and contraindications for this operation:

Indications.—Trephining should be practised in traumatic lesions of the head, if symptoms, clearly localized and circumscribed, persist, and, if all other means are unavailing, in the following cases:

1. In fractures of the vault, by perforation or with embedding of the fragments, whenever the injury to deep-seated parts occasions grave and continuous consequences, while remedial measures other than trephining may be impossible or inefficacious.

2. In fractures complicated with the perforation of foreign bodies of the thickness of the cranium, or with penetration into the superficial layers of the brain with persistence of symptomatic accidents, if the extraction of the foreign body can be effected in no other way.

3. In the different mechanical lesions of the head, complicated with grave and persistent cerebral symptoms, such as contusion and compression of the brain, or even prolonged hemiplegia with extravasation of blood or of pus, considered as circumscribed, provided that the lesion can be localized, and provided, especially, that active therapeutics remain insufficient.

Contraindications.—Trephining should not be attempted in cases of lesion speedily or necessarily fatal, nor in those presumed to be curable by other means:

1. When a foreign substance, lost in the deep portions of the brain, has passed beyond the reach of instruments.

2. If blood or pus, extravasated within the cranium, does not appear to form a focus in relation with the opening in the bone.

3. In every case of fracture not complicated with firm embedding of the fragments, nor with prolonged phenomena of compression or paralysis.

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[1] A series of photographs accompanying Dr. Bayard's article in the Canada Journal, leaves no room for doubt that the exfoliated bone is the odontoid process of the axis.—E. S. D.

4. In the condition of cerebral commotion or coma, with or without localized lesions.

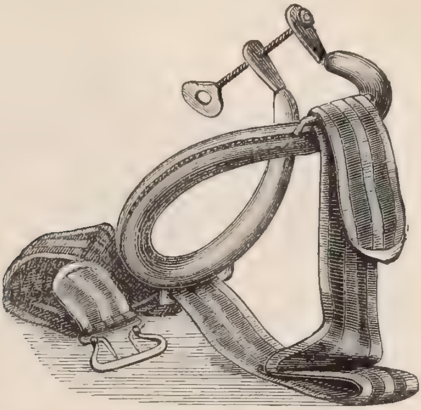
5. In indeterminate or epileptiform convulsions, not persistent and susceptible of cure.

6. In diffuse inflammation of the brain or its membranes, when clearly appreciable.—*Gazette des Hôpitaux.*

6.—*A New Splint for Fractures or Dislocations of the Patella.*

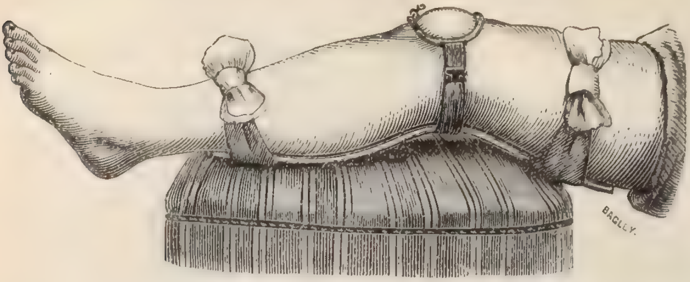
Dr. P. S. O'Reilly, of St. Louis, Mo, in the *Medical Archives* for January 7, 1870, thus describes a new splint, and the occurrence which gave origin to its contrivance:

On the night of the 16th of August last, Major George W. Gilson, one of the local reporters of the *Missouri Democrat*, and Major William Lee, Chief of the Metropolitan Police Department of St. Louis, were returning from a visit to the residence of Hon. C. P. Johnson, in a double-seated hunting-wagon, accompanied by two other gentlemen. On turning the corner of Walnut and Fifth Streets, to go eastward, the wheels of the vehicle slipped into a rut formed between the "Nicolson pavement" and the street railroad track, causing it to capsize, and precipitating the party with great violence upon the pavement. Chief Lee received a partial dislocation of the ankle-joint, and Major Gilson was injured in both knees, the patella of the left being fractured transversely a little above the centre, the upper fragment receding to an extent of three fingers' breadth, which is a little over two inches. I brought the fragments well together, and as I had no ring at hand (it being night), I dressed the limb after the manner of Woods's method, promising to call on the major early in the morning and change the dressing. Being otherwise professionally engaged, I could not keep my promise, and sent a note setting forth the facts to Prof. John T. Hodgen, who, never lacking in the *esprit de corps* for which the profession is proverbial, proceeded at once to apply the ring and posterior splint. I saw the patient in the afternoon. Inflammation had set in. The fragments were well up, and encompassed by their splint. He was comfortable, although he complained of the "bands causing cramps in his legs." Cold applications and mild antiphlogistic remedies were used, and in the course of five days the inflammation had somewhat subsided, and, had it not been, to use his own expression, for the "cramps in his leg," he would have felt "pretty easy." Upon examination, there was found to be a fissure between the fragments, and still the inner diameter of the ring was no larger than would be required for the sound patella. In searching for the cause of this separation, it was found that the ring was resting upon the upper fragment, instead of being above it. The ring was replaced, but in a few days the same displacement again partially occurred. I readjusted it, and upon watching for the manner in which this occurred—for the band was as tight as the patient could endure—it was found that, upon the twitchings of the muscles, the ligament of the rectus muscle became tense, tilted up the ring, and by this means allowed the fragment to partially escape from its iron casement. Pondering the matter over, and looking on the ring as a failure in this particular instance, I was led to wonder if surgical science really was futile in such cases. Here lay a man of fine physical organization, active mind and body, in the bloom of manhood, who had passed through two wars (the Mexican and late civil war) without any serious injury, likely to be crippled for life, despite of science, by a fall from a mere "dog-cart"—and the same thing likely to occur to others at any moment. To remedy the evil, it was necessary to overcome the presenting obstacles. As all fractures are usually treated by quietude and position, so should be this; and as these ends are accomplished by a well-adjusted splint, similar practice should prevail in this case. I therefore resolved to get up an apparatus, and the result of that determination is the splint which is here presented. Not only did it retain the fragments in position, but compressed them so perceptibly to the patient that he would remark, "I can feel the bones grating together."



It consists of a piece of rounded, tempered steel, and, as may be observed from the illustration, is of an oval, horseshoe shape, with the ends turned in and bent upward. The sides are depressed so that the part corresponding with the toe of the shoe is curved upward, to the extent of about half an inch, allowing it to ride over and upon the ligament of the rectus muscle, while the sides dip down and embrace the bone, and at the same time, by pressure at the insertion of the *vasti muscles*, it counteracts the action of the latter muscles; and yet it does not, by undue pressure upon the tendon of the rectus, cause the fragments to tilt up at the point of fracture, as must necessarily result when the pressure is made on this tendon alone, as is done by the simple ring. On the sides, slightly anterior to a line through the centre of the oval or horseshoe part, are fixed ears or loops for the reception of the band by which the *shoe portion* of the splint is held in position. Through the turned-up ends of that portion corresponding with the heel or calk of the horseshoe, is a thumb-screw by which the splint is compressed or expanded, as may be required. The splint being covered with chamois or soft felt, and a strap of silk or linen webbing, or smooth leather, with a buckle on one end, made to pass through one of the ears, render it complete for application. The injured limb being laid upon one of Day's curved posterior splints, well padded and a little wider than the leg, secured at each end by a handkerchief or roller bandage, the fragments being brought into position, the *Patella Splint* is placed over and made to encircle them, the strap carried under the leg and around the Day's splint, and run through the opposite loop or ear, is returned upon itself, and buckled sufficiently tight to keep the splint in position, but not so as to inconvenience the patient more than the case demands. By the thumb-screw the splint may be tightened or relaxed, as circumstances may require. Cold applications can now be applied *ad libitum.*

In case of dislocation of the patella upward, in rupture of the ligamentum patella from the tuberosity of the tibia, by an attachment of an elastic strap to the ears or loops, and passing it over the sole of the foot, the most perfect approximation may be secured.



MATERIA MEDICA AND THERAPEUTICS.

1.—*Danger of Injections of Perchloride of Iron in Sanguineous Tumors.* [New Orleans Journal of Medicine, October, 1869.]

A child ten weeks old, having three sanguineous tumors respectively on left cheek, left leg, and right lumbar region, was brought to Prof. Sautesson, of Stockholm. While in the country, the child had been for two weeks in charge of a physician, who was prevented from vaccinating the tumor on the face by want of virus, and instead applied collodion, but this produced no change.

The tumors were beneath the skin and evidently still growing. That on the face, the one of greatest importance, was an inch in diameter, and occupied more than half the thickness of the cheek, being situated midway on a line drawn from the ala-nasi to the lobule of the ear. Vaccination not promising any hope of success, and such procedures as excision, ligation and cauterization presenting serious objections, the choice was limited to acupuncture by galvanic needle, and injection of a coagulating liquid. The latter was preferred by the professor, and immediately, with the assistance of Prof. Abelin and Dr. Schlerg, the injection of a liquid, composed of ferri perchloridi six parts, and alcohol one part, was commenced. The injection was made with a subcutaneous glass syringe containing eight or ten drops of the mixture. The capillary tube was first introduced and directed vertically across the tumor, toward its centre, and only one-half of its contents injected, it was then withdrawn, and reintroduced horizontally, the point being directed deeper in the tumor. Before the second injection was completed, the operator was forced to withdraw the syringe on account of the appearance of threatening symptoms in the child. Death took place in about two hours.

Autopsy, on the day following death, revealed the following symptoms: The tumor was much lessened in size; its tissue, from being spongy, had become firm and solid from coagulation of the blood.

The surrounding veins (facial and its ramifications) were empty. The external and internal jugulars contained no clots in the superior portion, but as they approached the chest the contained blood was generally clotted. The clots became more and more firm in the subclavian, the superior vena cava, and in the right cavities of the heart. These were literally distended with coagulated blood. The left auricle contained a small clot, the left ventricle was empty. It is probable that, in introducing the tube the second time, one of the veins (perhaps a branch of the facial) was penetrated. The history of this case and its unfortunate termination suggest, as a precautionary measure against a similar occurrence, pressure between the tumor and the heart when performing the injection.—(*Monatssch.*) *bh. L'Union Médicale, May* 15, 1869.

2.—*The Therapeutical Value of the Sulphites.* By J. W. Miller, M. D. [Edinburgh Medical Journal, September, 1869.]

After recording a series of cases of typhus, eight hundred and eighty-four in number, and which were treated part with and part without the sulphites, showing no benefit whatever from their use, Dr. Miller adds:

Having seen the inutility of the sulphites in the treatment of typhus fever, I have not made any trial of them in the treatment of the other exanthemata. There remains, however, a class of diseases in which, I believe, they have been proved to possess great value—I mean, cases of purulent infection, or septicæmia. In midwifery practice, for instance, such cases are common enough. We have cases of abortion, in which the ovum is imperfectly expelled, and in which portions of the placenta are left adherent to the uterus, which shortly become putrid. In such cases the most serious consequences frequently ensue from the absorption into the blood of the putrescent material. In cases of adherent placenta, likewise, at the full term, the same peril is present. Under such circumstances we possess, I believe, in the sulphites a very effectual means of reducing the danger to a minimum. I have had several such opportunities in practice of testing the power of the sulphites, and with the most satisfactory results. One very marked case occurred to me within the last few weeks. After a very protracted and difficult case of labor, in which the child was hydrocephalic, the head requiring to be opened before delivery was effected, the placenta was found to be adherent. It required to be scraped from the uterus over its whole extent by the fingers, and doubtless numerous small fragments were unavoidably left here and there adherent, as was partly proved by the occurrence, a few days later, of considerable hæmorrhage, which was arrested by means of ergot. The woman lost much blood before the extraction of the placenta, was much exhausted by the long labor, and the uterus contracted very imperfectly. The discharges soon became putrid, and the patient was just in the condition to favor the absorption of the putrescent fluids by the uterine vessels. I put her at once under treatment by half-drachm doses of the sulphite of magnesia every fourth hour. For a few days she was in a high fever, but nevertheless she made a rapid and complete recovery, and was able to walk out of doors within three weeks after her delivery. Several other similar cases have occurred to me, to give the details of which, however, would be of little use. They were either cases of delivery at the full time, in which, from retention in the uterus of coagula of blood, or of fragments of the placenta, the lochia became putrid; or cases of abortion, in which the placenta came away piecemeal, and in which, under the employment of the sulphite of magnesia, the constitutional symptoms were either slight or entirely absent. To be of any avail, I believe the remedy must be given in considerable doses, from half a drachm to a drachm every fourth or sixth hour, and, still more, that its use must be commenced early in the case, either before or immediately on the appearance of the constitutional symptoms. In one case of abortion in which the sulphite was not given until after the development of symptoms of blood-poisoning, it had not the slightest effect in mitigating the course of the disease, and the case ended fatally.

3.—*Contribution to the Therapeutics of Laryngeal Diphtheria.* By Dr. J. Albu, Director of the Lazarus Hospital, Berlin. [St. Louis Medical and Surgical Journal, July 10, 1869.]

Laryngeal diphtheria being almost inaccessible to local treatment, inhalations and even tracheotomy being mostly unsuccessful, the latter in young children not even allowable, I have, in order to exert a dissolving influence on the diphtheritic membranes of the larynx, made injections into the latter of lukewarm lime-water. To this end I entered the canula of the injecting syringe from without, usually between the cartilaginous rings of the trachea, and at first threw only a few drops, but afterward, seeing that there was no danger in doing so, a whole syringeful, upward into the cavity of the trachea toward the larynx. This was never followed by suffocating paroxysms. Violent cough is induced, and the children usually at once eject shreds of diptheritic exudation. Thus far I have treated six cases in this manner, though only one successfully. The first five children so treated, all under five years of age, were already in the stage of suffocation, and lived, in consequence of the injection, as I am convinced, several hours longer than could have been expected. A girl of ten affected with decided laryngeal diphtheria, in whom tracheotomy was indicated, I have saved by the injection twice a day of a syringeful of lime-water, with the appropriate internal treatment (*Aqua Chlori, Decoct. Cinchonæ*).

We add to the above the following note by the editor of the *Wochenschrift*, Dr. Waldenburg, which is appended to Dr. Albu's report:

"In the treatment of diphtheria and croup, lime-water has hitherto proved in my hands also the best local remedy, and I employ it partly in the shape of gargles, partly for injections into the nose, and chiefly for inhalation in the atomized form by means of the steam apparatus. Children over three or four years old usually submit to these inhalations willingly. In cases in which this method is not applicable, I could not hitherto make up my mind to have recourse to the method recommended by Gottstein, of injections of aqua calcis *per orem* directly into the larynx, fearing suffocation. The method recommended above, by Albu, seems to show that the fear of suffocation in consequence of bringing liquids into the larynx is not quite well founded. But whether, having resolved upon this, the injection from without has any advantage over the injection *per orem*, seems *a priori* doubtful; Albu's present communication at least cannot yet solve the doubt. When the children are old and docile enough for the use of inhalations of lime-water, this method is decidedly preferable to every other local treatment."—*Berliner Klinische Wochenschrift*, February 1, 1869.

4.—*The Glycerine of Tannin.* By Meredith Clymer, M. D. [Medical Record, December 15, 1869.]

An incorrect formula for the glycerine of tannin is given in the *Medical Record*, November 1st, p. 396. It is officinal in the British Pharmacopœia of 1867, and the directions for its preparations are: "Take of tannic acid, one ounce; of glycerine, four fluidounces. Rub them together in a mortar, then transfer the mixture to a porcelain dish, and apply a gentle heat, until ample solution is effected."

Dr. Sydney Ringer first published his remarks on the employment of this preparation in *The Practitioner*, July, 1868. He says: "It appears to be but little known, and consequently but little used, while, in my opinion, it proves of great service in many diseases;" and he mentions ozœna; the reddish, sanious, or purulent discharging excoriations in the inside of the nose after scarlet fever or measles, with the consequent eczema of the upper lip; the thick, lumpy, greenish-black, often offensive, discharges from the nose, generally chronic and intractable; the thin sanious or purulent discharges from the ears, in weak, unhealthy children, particularly during convalescence from long illness; the early stages of eczema, when the skin is red, tumid, and weeping; the eczema which is limited to behind the ears of children; inflammations of the throat, after the acute symptoms have subsided, and particularly in those cases where there is general relaxation of the uvula and pharynx. In his "Handbook of Therapeutics," just published, Dr. Ringer adds to the list of affections in which this combination does good.

Having used glycerine of tannin since the appearance of Dr. Ringer's paper, in most of the disorders just mentioned, I can bear witness to its efficiency. I have also found, that in chafing (intertrigo), both in adults and children, it gives prompter relief than any other means. It will remove almost immediately the annoying itching of pruritus ani. Stuffing of the nose in syphilitic children, so characteristic and troublesome, and hindering sucking, is for a while relieved by it. In vaginal leucorrhœa it is of much and immediate service. The canal should be loosely packed with wads of cotton-wool, thoroughly moistened with it. Excoriations of the os tincæ, particularly where there is a boggy os, with a greenish-yellow or sometimes sanious discharge, may be dressed with glycerine of tannin. After cleansing the parts, a cotton wad well saturated with the preparation is to be placed completely round the os and vaginal cervix, and another wad, moistened with glycerine, over this; the dressing to be changed every twenty-four hours. In chronic gonorrhœa and gleet, injections of glycerine of tannin, diluted from one-half to two-thirds, will often prove quickly curative; it should remain in the urethra for five minutes.

Dr. Ringer writes: "In phthisis a frequent hacking cough is often dependent on the state of the throat, and can be allayed by this application. A good night's rest may be obtained by applying the paint just before going to sleep. A small quantity of morphia added to the glycerine of tannin still further increases its soothing, sedative power on the throat. The paroxysms of simple, uncomplicated whooping-cough may be most considerably lessened in frequency and violence by well sponging out the pharynx: it should be carried low down, and be brought well in contact with the epiglottis and the neighboring parts. . . . May be painted in the mucous membrane of the mouth in ulcerative stomatitis." ("A Handbook of Therapeutics," p. 221.) In throat-affections, I have not found it so efficient as other astringents, and by no means agreeable to the patient, though Dr. R. says, "It has the further advantage of not possessing a bad taste."

5.—*The Hypodermic Use of Ergot for Post-partum Hæmorrhage.* By Frederic D. Lente, M. D. [Medical Record, November 15, 1869.]

It occurred to me some time ago that one of the most important applications of the hypodermic method of medication would be in the use of ergot for the arrest of *post-partum* hæmorrhage. We here wish the most prompt effect of the drug attainable, while the irritability of the stomach, usually present in severe cases, is apt to cause a rejection of the remedy; or, if retained, the vital powers are at so low an ebb, absorption is tardy or perhaps entirely suspended. Intending to try the experiment myself at the first favorable opportunity, I suggested it to several friends engaged largely in obstetric practice, all of whom regarded the suggestion as valuable, but none of them, so far as I know, have put it to the test. I now desire to bring it more formally before the profession by the recital of a case in point.

Mrs. E. D. was delivered by me a few days since, after a natural labor, of a male child of more than the average size. The *placenta* followed soon after with but trifling assistance. Almost immediately after, although my hand never left its grasp of the uterus, fluid blood gushed forth in large quantity, and, within a very short time, the patient was blanched, and almost pulseless and insensible. The following means were resorted to promptly: Head on a level with the body, kneading the uterus with one hand while the other was passed into the vagina, and the fingers into the uterus, to turn out any clots which might be there, and to stimulate contraction, repeated twice; cloths wrung out of ice-water, applied in succession and at short intervals to abdomen and thighs; Squibb's extract of ergot in half-drachm and drachm doses; passage of a ball of ice into the uterus and held there, afterward into vagina; ice swallowed *ad libitum;* partial compression of aorta by the edge of the hand grasping the uterus; no stimulants. The uterus would contract firmly enough under these provocatives, but soon relax and become doughy. Finally, injected under the integument of thigh twenty-five drops of the ergot. Within four or five minutes, there seemed to be a more *tonic* contraction of the uterus; that is, it relaxed less frequently; at the end of thirteen minutes, the organ becoming more flabby, injected about thirty-five drops. But I did not notice any decided effect from this, unless it was exerted in the prevention of further hæmorrhage, for the patient had no further trouble.

In this case, the hæmorrhage had, in a great measure, ceased, it will be observed, before the injection was resorted to, therefore it is not demonstrated that the latter exerted any decided influence. In a similar case, I should use a half drachm, and repeat as often as necessary, until the specific effect of the remedy was obtained; the twenty-five drops do not make a greater bulk than about fifteen minims. Before commencing this paper I was not aware that ergot had ever been used hypodermically for any purpose. But, on taking up the October number of the *Richmond and Louisville Journal*, just received, I find an article alluding to an experiment of Prof. Langenbeck recently performed for the cure of aneurism by this method.

6.—*Chromic Acid in Favus and Herpes Tonsurans.* [Edinburgh Medical Journal, September, 1869.]

The editor, in commenting upon an article by Dr. Von Erlach, in the *Berliner Klinische Wochenschrift*, recommending the use of turpentine in parasitic skin-diseases, says these affections are most uncertain and capricious in their behavior, and hence the almost innumerable medicaments recommended for their treatment. Turpentine has long been used for this purpose, both in legitimate and irregular practice. His own experience with it, however, is not satisfactory; indeed, he positively asserts it is of no avail:

What, however, we have found perfectly effectual in all cases of favus and of ringworm, even the most obstinate, and therefore to be employed in all—because we do not know what cases will turn out obstinate—is the chromic acid in solution, one drachm to the ounce of water. The head should be shaved and cleared of all crusts, and then well painted all over with the solution. In hospital practice it is better to rub the solution well in at once, to allow it to remain on for an hour or so, and then, when it begins to get painful, to wash off the superfluous acid with cold water. In private practice it is better to paint the head all over once every night till a sufficient amount of reaction has been procured. This is evinced by some degree of inflammatory infiltration of the scalp, and occasionally by slight suppuration at various points. This shows that the acid has penetrated down to the living cutis, destroying all before it, both dead epithelium-cells and living spores infiltrated among them, or lying in the follicles. The irritation may then be soothed with a poultice, which also favors fresh cell-growth, or the head may be dressed with a solution of carbolic acid in lard, one drachm to the ounce. In our experience this has been the most effectual and the most easily-applied remedy, both for favus and also for ringworm, and one that is certainly far superior to oil of turpentine.

7.—*Carbolic-Acid Preparation.* [Chemist and Druggist—Pharmacist, December, 1869.]

Mr. T. A. Redwin, in a paper read before the British Pharmaceutical Conference, gives the following as advisable proportions in the use of carbolic acid:

As a rule, it is better to dissolve the crystallized carbolic acid (Calvert's) in the proportion of one part by weight of the acid to six of glycerine (carbolate of glycerine). In this state it can be diluted equally indefinitely.

In general, a *dose* of carbolic acid is 1 grain in an ounce of water.

As a *gargle*, 1 or 2 grains to an ounce of water.

As an *injection*, 1 grain to 4 ounces of water.

As a *lotion*, 15 grains to an ounce of water.

As an *ointment*, 30 grains to an ounce of benzoated lard.

As a *liniment*, 1 grain to 20 of olive-oil.

As a *plaster*, 1 part of carbolic acid to 3 of shellac.

The crystallized carbolic acid to be used as a caustic.

The carbolate of glycerine, as above, should be used in 1 or 2 drop doses.

Antiseptic oil, for abscesses, 1 part of acid to 4 of boiled linseed-oil.

Antiseptic putty, 6 spoonfuls of the antiseptic oil, mixed with common whiting.

Aqueous solution of carbolic acid is one part of acid to forty of water. (One ounce of acid to a quart of hot water well agitated and filtered.)

Sick-rooms, to disinfect: place a portion of the dissolved acid in a porcelain dish, and float it in a larger vessel of hot water.

Disinfecting purposes generally: 1 pound of *crystals* to 6 gallons of water. *Fluid*, 1 part to 80 of water. *Powder*, 1 ounce of crystals with 4 pounds of slaked lime.

For drains: take 1 pound of the fluid carbolic acid to 5 gallons of warm water.

Toothache is often cured with 1 drop of carbolate of glycerine, and diarrhœa arrested in half an hour with two drops.

In all cases of parasitic life it is advisable to commence with very dilute carbolate of glycerine.

8.—*The Physiological Action and Therapeutical Uses of the "Acidum Phosphoricum Dilutum."* [American Journal of Insanity, October, 1869.]

Dr. Judson B. Andrews, of the New York State Lunatic Asylum, gives the result of a large experience with the acid. After a brief discussion of the abnormal excretion of phosphates by the urine following mental exertion carried to the degree of fatigue, he speaks of the rational treatment to be adopted, viz.:

> To repair the waste which the nervous system has suffered, and again bring the secretions to a normal standard. To do this we have the means pointed out in the ingestion of food rich in nitrogenous elements, of substances capable of being converted into phosphoric acid, or of the acid itself. The former of these methods has always been resorted to by practitioners; the latter has attracted little notice.
>
> To demonstrate the physiological action of the remedy, pulse traces are presented taken by the sphygmograph, after the ingestion of the acid. The amount of acid taken varied from one to three drachms, and the traces were made at intervals of from fifteen minutes to one hour.
>
> Within the first interval there is an appreciable increase in the force of the pulsations, though there is little change in the number during the whole time of experimentation. The increase is most marked after the lapse of from one to two hours, and it is not till after several hours that the pulse returns to its normal condition. The first experiments I made upon myself, beginning with twenty drops, and continuing the use of the remedy in increased doses till the amount of four drachms was reached. The sensations experienced on taking from forty drops to three drachms were those of moderate alcoholic stimulation. There was slight pain through the frontal region, and a buoyancy and lightness of feeling rather agreeable. When a larger dose was taken, there was a feeling of drowsiness, an inclination to lie down, and an unwillingness to undertake mental labor. This continued for some hours. From these experiments, we may conclude that this remedy is a stimulant general in its character, but with a special tendency to the nervous system.[1]
>
> It also exercises a marked control over the vaso-motor system, as will be shown further on. We present some pulse-traces from patients who have been for some time continuously upon the use of the remedy. They are taken at random from a large number. In them the change in the tone of the circulation may be noted. Additional force is manifest in the heart's action in all cases where the traces were taken, and in others the same fact was evidenced by the general appearance. From our experience, we think it properly placed in the category of nerve-tonics.

After describing the dementia which follows outbreaks of acute mania, he remarks:

> This is a period of nervous exhaustion, of reaction from the increased mental and physical activity which marked the previous state of the disease. At this time tone and vigor must be supplied to the prostrated system, and, in the accomplishment of this, phosphoric acid is of material

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[1] The following extract is from the "Dictionnaire de Sciences Médicales": "The primary influence of phosphorus is exercised upon the nervous system, of which it increases the sensibility; from that it reacts upon the entire economy, and particularly on the circulatory system; the pulse is developed, heat augmented, the strength is increased."

service. Nature has made provision for the repair, in the generally unimpaired, and often greatly increased vigor of the digestive function, and it may be effectively assisted in this process of restoration by the employment of this acid, as a nerve nutriment. The traces marked number one of the pathological series, are those of a case to which this description will apply. Since being placed on the acid the change in the character of the circulation is strikingly apparent, and the improvement in the appearance and mental condition of the patient very marked. The congestion of the extremities and lips has given place to a more natural color, and the countenance wears the expression of a greater degree of mental vigor and activity. From this point to a full recovery, the steps are usually sure, and often rapid.

Cases are sometimes under treatment at the asylum, and more frequently in private practice, especially from among literary, professional, or business men, which are characterized by loss of mental power from excessive brain activity.

The patient is languid, unable to perform mental labor with the usual facility, is nervous, at times fearful, timid, and agitated; the memory is weakened, and permanent impairment seriously threatened. Examination reveals no organic lesion, but the symptoms are such as justly occasion alarm. Such cases have been improperly called by some recent writers, cases of cerebral paresis, a term too strong in its import, but expressive of the great danger which impends. For the recovery of these cases, relaxation from business and labor, and the use of the phosphoric acid, combined with some suitable tonic, generally suffices.

In cases where mental effort has been protracted till a sense of weariness renders its continuance difficult, a dose of the acid, from its stimulant effect, relieves fatigue, and seems to invigorate the mental powers, and prepare the mind for renewed exertion.[1] In the night-sweats attending consumption, and other exhausting diseases, this acid is employed with benefit, and has some advantages over the aromatic sulphuric acid, so generally used. It is much more agreeable to the taste, more likely to be tolerated, and does not constipate the bowels. The anti-scorbutic power of this acid is well settled.

Observation here confirms the views of Nelligan and others, that this substance exerts no direct influence on the generative function. It has thus been employed on theoretic grounds; but any favorable influence it has exerted has probably been owing to its general tonic effect. We have used it extensively, and in cases where this function was abnormally excited; and in no instance has its administration been suspended from this cause, or has any inconvenience resulted from its use.

In the administration of this remedy, one general principle should be kept in mind, viz.: not to exhibit it in cases of congestion of the brain, or in those in which there is an inflammatory action, either in the nerve substance or the meninges, as its stimulant effect might prove an aggravation to existing disease. In no case in which it has been given, has it disturbed digestion or proved an irritant to the stomach, even when its administration has been prolonged.

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[1] A professor in one of our medical schools in a letter to Dr. Gray, recently remarked: "Wonderful thing that phosphoric acid, and well named by me psychological lemonade. My lunch at noon (we dine at six) consists of rich cheese, bread, and a glass of phosphoric acid lemonade; and on that I have worked eight and nine hours a day, with my pen, for the past seven weeks in this hot weather, without headache or any depression. I never take over fifteen drops, and only once a day, and when fatigued. It is wonderful how quick it climbs into the anterior lobes, scatters capillary congestion, and satisfies the hungry tissue with its own pabulum."

9.—*Solubility of Diphtheritic Membranes.* [The Practitioner and Buffalo Medical Journal.]

In the November number we gave the results of experiments with solvents upon false membranes. From the following, it would appear that lactic acid is far superior to other agents:

MM. Brichetau and Adrian (*Union Pharm.*) have made a number of experiments to test the solubility of "false membrane" in various medicinal substances; and announce, among other things, the following results: A false membrane maintained for an hour in the midst of vapors of sulphate of mercury is not dissolved; it is only softened as by vapor of water; retained in a concentrated solution of pepsine maintained at a temperature of 30° C., it is not dissolved, but at the end of twelve hours; but if six to ten drops of lactic acid be added to the pepsine, the solution is accomplished at the end of eight minutes. Caustic acids (hydrochloric, sulphuric, azotic) do not dissolve false membranes. Acetic acid renders the diphtheritic membrane transparent, gelatinous, but does not dissolve it completely. Citric acid produces a similar effect. Lactic acid, in the proportion of two drops to five grammes of water, dissolved a tough membrane of the weight of twenty centigrammes in three minutes. With lime-water, the effect is still more rapid; lactate of lime is without action. Water alkalinized by soda or potassa, dissolves the membrane in eight or ten minutes, and better than their concentrated solutions. Bromine-water, and bromine *in statu nascente*, only disintegrate the membrane; bromide of potassium has no apparent action, salts of soda and potassa, such as sulphate of soda, sulphate of potassa, the bicarbonate, nitrate, etc., are without action, as also chloride of zinc and chromic acid. The chlorates of potassa and soda dissolve membranes, but slowly.—*L'Univ. Méd.*

10.—*Toxic Action of Quinine.* By Edward Garraway. [British Medical Journal, October 9, 1869.]

I was called last month to a lady, aged forty, in previous good health, who had been suddenly seized with œdema of the face and limbs, accompanied by an unusual erythematous rash. She had considerable uneasiness in the præcordia, and was in a state of great alarm. Certainly there was sufficient cause, for she was greatly disfigured, and her arms felt ready to burst. Her idea was, that she had been poisoned by a white powder, which she had procured at a chemist's, in mistake for quinine, and of which about a grain had been taken in a glass of wine. I taxed her with having eaten fungi, shell-fish, decomposing cheese, and other unwonted articles of food, but she pleaded guilty to none of these things. On bringing me the remains of the white powder, it proved to be pure sulphate of quinine. I repudiated the idea of this having done her any harm. After three or four days, the œdema and the rash subsided, but the skin of the face scaled off, and there was pealing of the hands and feet, as after scarlatina. My patient remaining somewhat enfeebled, I, unreflectingly, ordered a quinine mixture, by way of tonic. Two hours after taking the first dose—two grains—she sent for me, exclaiming, "Oh, you have poisoned me with quinine again." To my infinite chagrin and mortification, all the former symptoms recurred.

I doubt if I have omitted prescribing quinine any day for the last twenty years—in this locality it is largely needed—and this is the first instance in which I ever recognized any ill effect, beyond headache, resulting from its administration.

We find another case recorded in the same journal, of November 13th, by Mr. W. B. Hemming, as follows:

> A case similar to that related by Mr. Garraway, in the journal of October 9th, illustrative of the occasional idiosyncratic intolerance of quinine, has very recently occurred to me. It is the first time in twenty-five years' practice that I have seen such effects follow its administration; and, so far as my reading extends, I am not aware that they are mentioned by any writer on therapeutics as among the toxic qualities of that drug.
>
> I prescribed for a lady, advancing in life, who was recovering from a very severe accident, a mixture containing one grain of quinine in each dose. The next morning I received an urgent request to visit her, as she had "passed a sleepless night, and a rash had shown itself all over the body." I found this to be the case. The rash was as vivid as in scarlatina, and attended with intolerable and incessant itching; there was slight puffiness of the face, but no œdema elsewhere; a white tongue, and slight uneasiness in the præcordia. My patient immediately said, "I know this is the quinine, as it occurred twice before, and more severely, when I took it in France, some years ago." I made minute inquiry as to her diet, etc., but could not discover any thing to account for the rash. A highly-intelligent and cultivated friend who was with her at the time, confirmed what had occurred on the former occasions; and she said she had a clear recollection of having seen such an effect mentioned by a French author, accompanied by a theory as to its cause, but had not the book with her to refer to, and could not recall the name of the author. The rash and irritation were persistent for several days, and then slowly and gradually subsided, followed by universal exfoliation of the cuticle, which now, at the end of three weeks, is not quite completed.
>
> With the exception of fatigue from the sleeplessness, the result of the irritation of the skin, there was no symptoms of constitutional disturbance. My patient took only two doses of the mixture.

11.—*Hydrate of Chloral* [Translation from an article in the *Bulletin Général de Thérapeutique*, of November 30, 1869. By Dr. J. Cummiskey, Philadelphia].

The attention which the recent introduction of the hydrate of chloral has excited in the medical world, and the prospect, which it now encourages, of its superseding in a great degree the use of that most expensive and universally-used drug, opium, makes all practical information relative to its uses of the highest importance to the physician. The article, from which this extract is taken, is, I presume, from the pen of the accomplished editor[1] of the *Bulletin de Thérapeutique*, and is quite an exhaustive one. The conclusions which he has arrived at, and which are based upon extensive observations of the effects of hydrate of chloral, are given below, and will be found an epitome of all that is now practically known of this important article of the materia medica.

> 1. Hydrate of chloral is a powerful sedative to the sensitive and motor nervous system.

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[1] Bricheteau.

2. If hydrate of chloral be not crystallized and very pure, it will have no effect, and may be very dangerous. Its purity may be ascertained by adding to its solution a little caustic potassa, which should disengage the vapor of chloroform, without discoloring the solution.

3. Hydrate of chloral should not be given to adults in a larger dose than from five to six grammes,[1] and from one to two grammes to children.

4. The preparations of hydrate of chloral should be freshly made, to give satisfactory results, as such preparations change and lose their strength.

5. Hydrate of chloral may be administered either by the mouth or by the rectum, the former, however, is preferable. It is dangerous to give it hypodermically to man.

6. Hydrate of chloral must not be administered to patients suffering from organic diseases of the brain or heart.

7. Hydrate of chloral produces sleep, and anæsthesia by the production of chloroform in the blood, through the action of the alkalies of the blood.

8. Arterial tension increases under the influence of chloral, and frequency of the pulse is produced at the same time. This tension is shown by the sphygmograph, to decrease, however, as soon as the patient awakes,

9. After administering chloral, the urine is found to be neutral in reaction, and when first boiled with Fehling's solution, it does not give any sign of reduction; but the next day, when chloral is excreted by the kidneys. the urine is found to be denser and reduces the copper solution, giving the impression that glycosuria exists, which, however, is not really the case.

10. Chloral rarely vomits or purges.

11. The temperature is lowered by ordinary doses of chloral, showing it to be a refrigerant remedy.

12. Chloral diminishes cutaneous perspiration, and makes the skin therefore drier than usual.

13. Chloral in the production of anæsthesia has the advantage over chloroform, of being administered in exact quantities, while chloroform being inhaled, cannot be given with the exactitude desired.

14. The action of chloral is exactly the same as that of chloroform, but it follows more slowly and lasts longer.

15. In some persons chloral produces a muscular and mental agitation, resembling that of alcoholic intoxication, but is neither so disgusting nor disagreeable as the latter.

16. The sleep produced by chloral is in most cases accompanied by a very decided anæsthesia, and very rarely by hyperæsthesia.

17. The anæsthesia is in proportion to the dose given, and in the dose of from two to five grammes according to the age, it is complete, and permits the application, without pain, of the Vienna paste, and even teeth may be extracted.

18. With opium, chloral bears a favorable comparison. With the administration of opium, vomiting and sickness of the stomach, are often produced, the appetite is seriously affected, the body is stimulated and heated, the bowels are constipated, the transpiration excited, the sleep is slow and heavy, and even after awaking a sense of drowsiness is experienced for a time. After chloral has been taken, no sickness or vomiting is experienced, no constipation; the appetite is increased, the skin becomes drier and cooler, no heaviness or drowsiness follows, and it may be taken several days consecutively.

19. A dose of from three to five grammes of chloral may be repeated two or three times in a day without inconvenience, and several hours of sleep may each time be obtained.

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[1] 15.438 grains Troy.

20. Therapeutically, chloral is the first among those sedatives or anæsthetics administered by the stomach, relieving promptly the violent pains of gout, allaying the atrocious sufferings of nephritic colic. In natural labor, chloral may be used to assuage the pain, to facilitate obstetrical operations, and to combat eclampsia.

21. In conclusion, chloral is the most prompt and efficacious remedy in intense chorea, where it is desirable to subdue rapidly the extreme agitation which threatens the life of the patient.

12.—*The Composition of Chlorodyne.* [Medical Times and Gazette, January 8, 1870.]

Mr. Edward Smith reports, in the *Pharmaceutical Journal* for January, an inquiry into the composition of this well-known secret remedy. Hitherto, of the formulæ which have been published, two—one by Dr. Ogden, the other by Mr. Squire—have attracted most attention in this country. The difference between these lay essentially in the presence of Indian hemp and capsicum as indicated by Ogden, their absence in the formula given by Squire. But besides this, the proportion of morphia, as given by the two authorities, differed greatly. About the three important ingredients—chloroform, morphia, and hydrocyanic acid—there can be no doubt; no more can there be about oil of peppermint and treacle. The question is, whether any thing else exists in the compound. Mr. Smith thinks there is no Indian hemp, because the alcoholic extract is soluble in water; but then there is capsicum, as, after the chloroform and ether, which also give pungency to the mixture, have been distilled off, the substance left behind has a hot peppery taste. He seems to have taken much pains with the analysis. Here is the composition he assigns to chlorodyne:

℞ Chloroformi f. ʒ iv.
Morphiæ mur. gr. xx.
Æther. rectif. f. ʒ ij.
Ol. menthæ pip. ♏ viij.
Acidi hydrocyanici dil. f. ʒ iv.
Tinct. capsici f. ʒ vj.
Mist. acaciæ f. ℥ j.
Theriacæ ad f. ℥ v.

This is not quite so dark as the original, as no caramel is used. Mr. Smith suggests for it the title Liquor Chloromorphiæ Co., as not likely to be confounded with that of any other compound.

13.—*Tetanus and Curare; Notes on some Unpublished Clinical Experiments.* [Gazette Med. Ital. di Lombardia.]

These experiments were made on horses, as well as on man, by Dr. Felice Dell' Acqua, who is known in Italy for his experiments made in the treatment of tetanus with curare, and who has published several pamphlets on the subject.

The experiments related in this paper are six in number—three made on man, and three on horses.

In the three experiments made on horses, some relief was noticed in all cases, but, nevertheless, all of them died. The curare was used by hypodermic injections; the quantity administered in each case being from 1.50 to 2.20 grammes (22 to 40 grains). Among the favorable and amending symptoms, a diminution of the trismus, of the dysphagia, and muscular relaxation were observed. The tetanus was traumatic in two cases, and rheumatismal in one.

The three patients submitted to this medication were: a robust woman aged twenty-four; another, a very debilitated subject, aged thirty-four; and a young girl of fourteen. In the first case, the cure was effected rapidly; in the second, there was a marked improvement, followed, however, by death; in the third, the effect of the medicine was perfectly null. In a fourth case reported by Dr. Gianelli *(Di Lombardia)*, the effect of the remedy was so favorable that it evidently prolonged the life of the patient, and procured great relief in the intensity of all the symptoms.

The conclusions are thus summarized by the author:

1. Considering the daily failure of ordinary therapeutics in the treatment of tetanus;

2. Considering that, in sixteen cases of tetanus in man treated with curare, six have been cured, while in twenty-one cases treated without curare at the Maggiore Hospital in Milan, four only have been cured;

3. Considering that, although it is true that curare administered by us to some tetanic horses has not acted successfully, either because the disease was too advanced, or for some other reason, it is nevertheless true that Sewell and Harley have cured two horses out of three of the same disease, and with the same therapeutic agent;

4. Considering the success obtained at the Maggiore Hospital at Milan, in a case of partial tetanus;

5. This fact being accounted for, as well as the numerous symptomatic remissions obtained, even in the unsuccessful cases in man and animals; seeing that in the three cases of tetanus equini reported by me, and in the case of human tetanus observed by Dr. Gianeli, if curare could not prevent a fatal termination, it was at least useful in amending the painful symptoms characteristic of tetanus, and produced in several cases a prolongation of life (a circumstance sometimes very important for man);

6. Seeing, finally, that the many remissions in the symptoms have appeared suddenly and constantly after the applications of curare, that they have been continued sometimes to a period much longer than that of tetanus treated without curare, that the muscular relief produced by curare did not comprise only the muscles of relation and animal life, but also those of organic life (micturition and defecation).

I am led to believe that, in human as well as in veterinary medicine, the rapid antispastic and paralyzing action of curare should be attempted in the treatment of tetanus; and that, if it be not a sure means of cure, at least it is a sedative and an alleviator of the symptoms.

Remarks.—It is to be regretted that Dr. Dell' Acqua did not tell us in what doses he used the curare, in the cases where it was used on man; it is important, when using such a powerful agent, that the quantity used should be explicitly stated. It is still more necessary in this case, for the reason that the dose is mentioned for horses, but not for man. Now, if we give a man curare in proportion to that given to the horses, we are sure to kill him. I have used curare after the experiments of Cl. Bernard, and found it to be a most powerful agent, producing paralysis of motion and a total muscular relaxation. I have used it also in mammifera, in birds, and batrachians, but its ultimate action was best demonstrated in a turtle.

Every one knows that no animal is more tenacious of life than a turtle. One of these animals may be taken, its head chopped off, and it will, notwithstanding that, move for three or four days. Selecting a terrapin, weighing two pounds and very lively, I introduced a small fragment of curare, about the size of the *point* of a pin, under the skin of the left hind leg, and after six minutes the animal was apparently dead; if the dose had been given in solution instead of in the form of a hard fragment, death would have come much sooner. But this only shows what a powerful agent curare is.

It is said that, in India, a single arrow loaded with the poison is sufficient sometimes to kill an elephant. We find also in Watterton the narration of the death of a man poisoned by curare.

This is not the place nor the time to extend these remarks; I have done this much, simply because curare is still in this country a comparatively untried remedy. The best work in reference to the subject (Claude Bernard, "Des Effets des Substances toxiques et medicamenteuses") has not been translated in the English language, and the inexperienced, on reading that curare is almost a certain remedy in treatment of tetanic affections, may use it upon too large a scale, if they take the proportion used by Dr. Dell' Acqua on horses, as a comparative guide, and do mischief where they intend only to do good.

P. DeMarmon, M. D.

Miscellaneous and Scientific Notes.

We are pleased to announce to our readers that the article in the present number of the Journal, by Dr. A. H. Smith, on the Therapeutic Uses of Oxygen Gas, received the prize of two hundred dollars awarded by the Alumni Association of the College of Physicians and Surgeons of this city, March 1, 1870.

Dr. Dieulafoy's Subcutaneous Exhauster.—Dr. Dieulafoy has just invented a most ingenious instrument, destined no doubt to render important service in the daily practice of medicine and surgery.

All practitioners know how difficult it is to recognize the presence of a purulent collection, concealed underneath the muscles of an aponeurosis; the observer remains undecided both concerning the presence and the nature of a liquid which for some time is revealed neither by any notable tumefaction nor by fluctuation.

The exploring trochar was invented with a view to overcome this difficulty, but it is at once too large, compared with those fine needles used for subcutaneous injections, and too small to permit of the liquid being drained off in all cases. To remedy this inconvenience Dr. Dieulafoy has constructed long canula-trochars, so exceedingly slender in size and form, that the most delicate organs may be traversed by them without suffering more than from the passage of acupuncture needles, the perfect harmlessness of which has so often been demonstrated. This canula-trochar, furnished with two slits at the end, is introduced in search of the supposed liquid; it is easy to make the vacuum by means of the piston of a syringe, and the operator is at once informed of the presence, seat, and nature of the collection. Its seat is determined by the direction and depth given to the canula; afterward its real nature may be ascertained by a microscopic examination.

As regards the treatment, the articular effusions may be evacuated, without the slightest danger of a single air-bell being introduced, or the smallest puncture capable of occasioning traumatic accidents. Would not an analogous treatment be applicable to effusions taking place in the pericardium?

Experience will show if this operation be not preferable to the paracentesis of the cardiac covering, so difficult and dangerous to perform.

This instrument may also be used for the aspiration of urine in cases of retention, for the extraction of the liquid in encysted pleurisies, for abscesses by congestion, etc.

The draining of the liquid may be rendered continuous by the action of two valves, placed at the end of the syringe, which alternately open and shut.

If the injection of any liquid, tincture of iodine, or alcohol, be judged necessary, it may at once be proceeded with, without displacing the instrument; the aspiration of the liquid to be injected being made by an under tube.

The subcutaneous exhauster is also destined to expel the gases which accumulate in such large quantities in the intestinal occlusions, and which become, in certain circumstances, one of the obstacles to the reduction of certain kinds of hernia.

In short, this instrument can render good service, when it is determined to make discharges of blood, either by inserting the needle into a vein or artery, or by draining and disgorging hyperemized parts, for the relief of persons suffocated, strangled, or asphyxiated, that is to say, before life has become extinct, etc., etc.

The trials made in the principal hospitals of Paris, during the month of November, have been so satisfactory, that it is easy to predict that the use of the subcutaneous exhauster will soon become general.

American Medical Association.—The twenty-first annual session will be held in Washington, D. C., May 3, 1870, at 11 a. m. The following committees are expected to report: On Cultivation of the Cinchona-Tree, Dr. Lemuel J. Deal, Pennsylvania, chairman. On the Cryptogamic Origin of Disease, with special reference to recent microscopic investigations on that subject, Dr. Edward Curtis, U. S. A., chairman. On the Doctrine of Force, Physical and Vital, Dr. John Watters, Missouri, chairman. On Variola, Dr. Joseph Jones, Louisiana, chairman. On the Relative Advantages of Syme's and Pirogoff's mode of amputating at the Ankle, Dr. G. A. Otis, U. S. A., chairman On a National Medical School, Dr. F. G. Smith, Pennsylvania, chairman. On Commissioners to aid in Trials involving Scientific Testimony, Dr. John Ordronaux, N. Y., chairman. On the Climatology and Epidemics of—Maine, Dr. J. C. Weston; New Hampshire, Dr. P. A. Stackpole; Vermont, Dr. Henry Janes; Massachusetts, Dr. H. I. Bowditch; Rhode Island, Dr. C. W. Parsons; Connecticut, Dr. E. K. Hunt; New York, Dr. W. F. Thoms; New Jersey, Dr. Ezra M. Hunt; Pennsylvania, Dr. D. F. Condie; Maryland, Dr. O. S. Mahon; Georgia, Dr. Juriah Harriss; Missouri, Dr. George Engleman; Alabama, Dr. R. F. Michel; Texas, Dr. T. J. Heard; Illinois, Dr. R. C. Hamil; Indiana, Dr. J. F. Hibberd; District of Columbia, Dr. T. Antisell; Iowa, Dr. J. C. Hughes; Michigan, Dr. Abraham Sager; Ohio, Dr. T. L. Neal; California, Dr. F. W. Hatch; Tennessee, Dr. B. W. Avent; West Virginia, Dr. E. A. Hildreth; Minnesota, Dr. Samuel Willey; Virginia, Dr. W. O. Owen; Delaware, Dr. L. B. Bush; Arkansas, Dr. G. W. Lawrence; Mississippi, Dr. W. Compton; Louisiana, Dr. L. T. Pimm; Wisconsin, Dr. J. K. Bartlett; Kentucky, Dr. J. D. Jackson.

On Veterinary Colleges, Dr. Thomas Antisell, D. C., chairman. On Medical Ethics, Dr. Lewis A. Sayre, N. Y., chairman. On American Medical Necrology, Dr. C. C. Cox, Maryland, chairman. To memorialize State Medical Societies, Dr. N. S. Davis, Illinois, chairman. On Nomenclature of Diseases, Dr. F. G. Smith, Pennsylvania, chairman. On Medical Education, Dr. T. G. Richardson, Louisiana, chairman. On Medical Literature, Dr. J. J. Woodward, U. S. A., chairman. On Prize Essays, Dr. Grafton Tyler, D. C., chairman. *Voluntary* communications will be presented by Dr. John Curwen, Pennsylvania, on the Proper Treatment of the Insane. Dr. Nathan Allen, Massachusetts, on the Physiological Laws of Human Increase. Secretaries of all medical organizations are requested to forward lists of their delegates as soon as elected, to the permanent secretary. Any respectable physician who may desire to attend, but cannot do so as a delegate, may be made a *member by invitation*, upon the recommendation of the committee of arrangements.—*Prog. of Med.*

THE MEDICAL CORPS OF THE NAVY.—"There have been fifty-three resignations among the medical officers for the last four years, from a corps of two hundred (but one of them discreditably to the officer), and only one of them has been filled, and it is impossible now properly to officer the ships and hospitals."

What more striking commentary could be made on the status of the medical officers in our Navy, than the above single sentence extracted from the report of Dr. Wood, chief of the Bureau of Medicine and Surgery, to the Secretary of the Navy. If the medical men young and old of the country, will only maintain their present attitude toward the Navy, the martinets who now control affairs, will soon be forced to take a more reasonable and honorable view of the condition of things than they now entertain.. No man can enter the medical corps of the Navy at the present time without disgracing himself and his profession.

Death from Chloroform.—Dr. J. F. Miner reports, in the *Buffalo Medical Journal*, the following case of apparent death from chloroform, which is of value, in a statistical point of view at least, and has some features of interest connected with the general questions of, to whom, and under what circumstances, is it safe and proper to administer chloroform:

October 20th.—E. B., aged about forty, received injury in coupling cars, the right hand being crushed between the car-bumpers. He was brought into my office, and upon examination was told, what was before quite apparent to him, that amputation of the forearm would be necessary.

He requested chloroform to be given, and I commenced its administration, while my private students arranged for the operation. The chloroform was given by dropping it upon a napkin, and holding it at sufficient distance to allow ample atmospheric air. After breathing it for a few minutes, he became talkative, and finally considerably excited, requiring restraint. He soon had a condition of rigidity of the muscular system, drawing back the head, as in partial convulsion. This condition attracted my attention and caused me to withdraw the chloroform, though he had, the second before, talked loudly and profanely, and did not appear enough under its influence for me to think of permanently discontinuing it. His appearance was now peculiar, and cannot be described by words. I noticed that there was something in his respiration and general condition which I had never before observed in patients inhaling the vapor of chloroform. I had merely time to say to my assistants, "His pulse is very weak," when I was obliged to finish my sentence by saying, "It has stopped." Respiration ceased after one or two short inspiratory efforts, and my patient was dead.

Such is a brief history of what occurred before *post-mortem* examination, in which I was assisted by a number of my professional friends.

Prof. T. F. Rochester, at my request, has kindly furnished the following *post-mortem* appearances, and the results also of a careful microscopic examination of the tissues of the heart:

Autopsy.—Six hours after death. Body of a well-formed man, slightly muscular, about forty years of age. Weight estimated at one hundred and forty-five pounds. Surface cold. Cadaveric rigidity slight. Hæmatic suggilation very marked. Hand and lower forearm terribly crushed, lacerated, and denuded.

Sectio Cadaveris—Thorax.—Lungs collapsed, no adhesions, crepitant, pallid—healthy. Pericardium contained about one ounce of straw-colored serum. Two white spots (maculæ lacteæ) on cardiac reflexion of right ventricle, the larger an inch in diameter. *Heart.* Right auricle and right ventricle enormously distended with dark and fluid blood. Fatty deposit about base of heart, a little more than common. Ventricles *red*, but doughy and inelastic, and feeling like tallow when punctured with a tenaculum. *Heart opened.* A large quantity of dark fluid blood escaped from both auricle and ventricle of right side; left auricle and ventricle *empty.* The tricuspid valve and the aortic and pulmonic valves were normal. The mitral valve was functionally adequate, but was the seat of a slight abnormal deposit, probably fibrous. Weight of heart, emptied, ten and a half ounces. The walls of the auricles and ventricles were about normal in thickness, but the belly of the left ventricle slightly exceeded half an inch. The cavities were somewhat more capacious than usual. The muscular fibre looked pretty well, but was soft and *mashed* under the finger like tallow.

Microscopic Examination.—Portions of the right and left ventricles, and of the columnæ carnæ of the left ventricle were inspected: the muscular fibre was very distinct, but looked like a wax cast of the same. It was very thickly interspersed with fat-granules, and oil-globules were very abundant in the field. The liver was healthy; the kidneys normal, but engorged with dark fluid blood.

Remarks.—The extreme fluidity of the blood, and the fact that both the right auricle and right ventricle were enormously distended, is, to say the least, peculiar—and affords much room for speculation. There was fatty muscular degeneration to an advanced degree, and yet it is very doubtful if this could have been detected by careful physical exploration, conducted at any time before the occurrence of the sad accident.

In most of the deaths by chloroform, where *post-mortem* examinations have been made, "fatty degeneration of the heart" is reported. This condition must be much more frequent than is generally supposed; and where there have been no signs or symptoms to indicate its existence, it cannot be discovered by manipulation or auscultation

Fatty degeneration of the heart, without other change, is a pathological condition, which usually gives during life no signs adequate to its discovery. If we could know that such disease was present, we might refuse chloroform and ether, and choose the suffering incident to operation rather than the risk of anæsthesia. But a well-marked case of fatty degeneration of the heart has dangers which even anæsthesia might lessen. All surgeons are familiar with the case reported by De Sault, who lost a patient about to be lithotomized. He was supposed to have died from sheer fright, when this distinguished surgeon only marked the line of intended incision with the thumb-nail. If this patient had been inhaling anæsthetic vapor, there would have been no doubt, that *it* was the agent which produced the fatal result.

In the case reported, the fright and excitement which the injury and anticipated operation caused, was adequate to produce instant death, in a case of fatty degeneration of the heart, or in any other condition of this organ where its strength or function was greatly interfered with. It is a common occurrence to see patients with structural disease of the heart drop down and die suddenly from the slightest causes of excitement and increased circulation. It may be that the increased heart-action, caused by excitement and fear, is as dangerous in such cases as the anæsthetic. Formerly we used to hear of *fear* and mental *shock* proving instantly fatal, but no mention is made in those cases of the condition of the central organ of the circulation. The histories of persons being sentenced to be bled to death, who actually died on hearing the water trickle into the basin, which they supposed to be blood issuing from their veins, after their arms had been slightly pricked, although no vessel had been opened, are familiar to all. If we had given our patient no anæsthetic, and the result had been the same, we should have been reasonably well satisfied with attributing the death to mental shock, or mental shock joined with the shock of injury, and if not thus satisfied, we should have regarded the explanation as complete when the condition of fatty degeneration of the heart was taken into consideration. It *seemed* at first a case of unmistakable death from chloroform, and the *post-mortem* examination was commenced with the expectation that no organic change would be found to explain the result. The history of death from chloroform would seem to leave no doubt of its having proved fatal in a considerable number of cases, where no fault in administration or disease of patient could be urged in explanation or extenuation. It is perhaps as desirable to make careful discrimination, and exclude from our list of deaths from chloroform all cases which do not rightfully belong to it, as it is to trace to it all deaths which it causes. We want to know the amount of risk involved in the inhalation of chloroform in conditions of perfect health, and what effect it has, also, in conditions of disease. But few of the cases of death from chloroform have been subject to careful *post-mortem* examination, and consequently their histories may be said to be in great degree imperfect.

DEATHS FROM CHLOROFORM. By JOHN MURRAY, M. D., F. R. C. S. E., ETC.—On the 11th of this month a young man was admitted into the Ovens District Hospital for a disease in one of his fingers, which it was decided to amputate. As the patient was very nervous, chloroform was employed, at his own desire, the administration of which resulted in his death.

An inquest was held, and a lengthy examination of the medical gentlemen attending the case was made; the verdict stated that death resulted from stoppage of the heart's action. Before the chloroform was used, the usual examination of the patient's heart and chest by the stethoscope was made, and nothing abnormal discovered.

On the 20th of March, a patient in the Lying-in-Hospital died during the administration of chloroform, while undergoing an operation for the relief of prolapsus uteri. At the *post mortem* nothing unusual was discovered, except a slightly flabby state of the heart. The verdict attributed no blame to any person.

A third case occurred in the Melbourne Hospital about a year and a half ago, when the *post-mortem* examination disclosed a diseased state of the heart, but the description was very imperfect.

Now, in the first of these cases, it appears that the usual precautions were taken, and nothing unfavorable discovered, and yet one of the medical witnesses stated that the right auricle was in a state of fatty degeneration, which might have been discovered by an accurate stethoscopic examination, and yet was not.

What, then, was the cause of death? Was the stethoscopic examination of the heart, lungs, etc., so minute and accurate as to preclude the conviction of serious disease in these organs? or did death occur from the bad quality of the chloroform? That such might be the case is evident from the noxious character of the impurity occasionally found in chloroform.

There is an impurity not unfrequently found in this drug, and altogether unsuspected. . . .

Whether any of the three deaths occurred from the use of impure chloroform or not, would be ascertained by the acid test, which ought always to be tried when the article is supplied to a public institution. Every druggist should also test his chloroform, so as to guard against accidents in private practice.

In connection with this subject, I may remark that no detailed report of the medical evidence at the inquest was furnished in any of the Melbourne journals, but in the local paper of the Ovens, which is not seen by the profession generally, a full report is given.

Whether medical coroners are to be continued or no, a full official report of the medical evidence ought to be furnished in every case, and published in the *Government Gazette*, for the benefit of the public and the profession.—*Australian Medical Gazette.*

M. L. LABBE communicated to the Imperial Society of Surgery, March 31, 1869, a case of this. The subject of it was a man aged forty-two, who had fractured both bones of his left leg January 19, 1869. Six days afterward tetanic symptoms came on. To quiet the spasm, the next day chloroform was given, but after a few inspirations the pulse ceased, and the patient presented all the apparent signs of death. Artificial respiration was resorted to, the tongue drawn out, and after a short time the circulation and respiration were reestablished. The patient was then put to bed, apparently in a satisfactory condition. But a moment afterward the respiration suddenly ceased, and, despite all the usual efforts, life became extinct.—*Journal Hebdom., April* 30, 1869.

NEW YORK, *December 3d.*—Mrs. Sarah A. Kruger died at the Washington Hotel, Fourth Avenue, on Wednesday night, from the inhalation of chloroform, which her husband bought for her at a drug-store without a prescription. The two had not lived together for a year, and Dr. Cormins testified that she had told him she intended to take her life in that way, owing to her domestic troubles. The jury found that her death was occasioned by inhaling chloroform to relieve headache.—*Boston Daily Journal.*

American Medical Periodicals.—We copy without comment the following article, from the *Medical Times and Gazette*, of London, of January 8, 1870:

Nowhere do political newspapers thrive better than in America, nowhere have they greater weight and influence, and yet the medical newspaper exists only in the most rudimentary form. Nor is it the absence of an organ speaking for, and to, the whole profession in America, which constitutes the sole characteristic of the medical press; the kind of its contents is at least equally noteworthy. The average American journal is lamentably deficient from a scientific point of view; ordinarily it rests far too much on others, far too little on itself. Up to a very recent period there was but one journal, a quarterly, which could be pointed at as a fair exponent of American medical science, and in certain respects, at all events, it was conducted on principles of short-sighted policy such as could not prevail even in benighted England. But we are heartily glad to note a great and a marked change for the better, although much remains to be done. We look upon the establishment of the New York Medical Journal as an era in the medical history of the States. One thing it has done—it has excited a higher respect for American medicine than was before prevalent in this country. As a rule, American practitioners are clever and suggestive, ready to strike out new paths in the art of healing, but, probably from their imperfect training, they are rarely skilful penmen, and their attempts seldom go beyond the recording of a curious case. But, in the great American cities, men are now springing up well educated in every sense of the word, and well fitted to advance the knowledge of our profession in every way. American physicians held too long by the brilliant but illusory French writers; now they are turning to the more solid Germans with a corresponding advantage. In another respect most American writers are very shaky—that is, with regard to their pathology, and here it is their writings are most barren; but, in this also, they show signs of improvement, and by-and-by we may expect the vast quantity of material afforded by such a country to be fully utilized. Occasionally in some of the Southern journals we come across most interesting details as to malaria and such-like matters, and the *St. Louis Medical Journal* has been the means of introducing into the country some admirable translations of European authors; but still there is a defect.

One thing is constantly giving rise to discussions in America—that is, their code of ethics. We are inclined to think that too much is said about ethics for very much to be done. Now in this country we have no written code of ethics, yet we have, probably, fewer offenders against the laws of gentlemanly conduct than there are over the water, code of ethics and all. Their plan is something like making a man moral by act of Parliament. In this country the leading medical journals really constitute the code of ethics. They have obtained the respect of the profession at large, who trust them and their decisions. There is nothing like this in America. Very few American journals are weeklies; most of them are monthlies or quarterlies, and consequently are not in a position to act as professional arbitrators. Yet some of the weeklies show signs of ability, such as, if they received a more general support, might develop into great professional usefulness. To take an instance: The *New York Medical Gazette* has recently published a series of articles on Hahnemann and homœopathy, given in wretched driblets, it is true. so that, to benefit by it, the reader would have to wait patiently to the end, and then study the whole series together. Now these articles are unusually able, but, cut up as they are, their utility is lost. In another weekly we might mention, there is a slanginess, a taking after the political press, which renders it, to "Britishers" at least, unpleasant and unattractive. The frantic efforts made to procure subscribers are another feature which detracts from the weight of many American periodicals. All sorts of inducements but the right ones are held out. We fancy if they did as we do here—secure the best writings of the best men—offers of bonuses and composition fees would not be needed. There must surely be in New York the means of turning out a weekly as the monthly journal is turned out—one, that is to say, which relies on its own intrinsic merits, and not on pickings and stealings from its neighbors—and such a journal, we think, would be a boon to the profession at large; but such an undertaking requires general support.

The amenities of the medical press in America are sometimes amusing enough. One gentleman apologized for the non-appearance of his journal one month by publicly announcing that he had no time to attend to it. On another occasion the same individual accounted for a lot of blunders by the fact that he was off for a holiday when the journal was going through the press. What would our readers say were we to treat them thus? Upon the whole, we are hopeful of the condition of the American medical press; its defects are those which time will remove. And, although we cannot quite agree with the enthusiastic gentlemen who hold that we old-country men will by-and-by pass over to America to learn our profession, still we do believe that the medical press is increasing in influence, and most deservedly so, in America, and we should be glad if a more generous support would enable those who are doubtless both able and willing to turn out a paper which shall vie with, or even excel, its British congeners.

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Original Communications.

Art. I.—*Notes on the Physiology and Pathology of the Nervous System, with Reference to Clinical Medicine.* By Meredith Clymer, M. D. Univ. Penn., F. C. P. Phil., etc., etc.

It is proposed in these papers to summarize the recent investigations into the physiology and pathology of the nervous system which have a bearing on clinical medicine. Quite lately much of worth has been done in this direction. New and nicer methods of examining the intimate structure of the brain and spinal marrow in health and in disease, have unsettled many traditional notions concerning several classical disorders, and fixed the connection between symptoms and lesions. Thanks to the better studies of those who have busied themselves with these subjects, we have now satisfying and trustworthy information, where before confusion and uncertainty, or, at best, happy conjecture, ruled. There has been patient and productive working; facts have been largely and heedfully gathered, and rightly used; sagacity has tempered zeal; and more concern has been shown about practical results than engaging hypotheses. It is not, therefore, claiming too much to say that art is a great gainer by these scientific inquiries. The outcome is a less faulty pathogeny, with a truer understanding of many common affections of the nervous centres, and surer means of detection and treatment.

I.

SCLEROSIS OF THE NERVOUS CENTRES.

Virchow says, "Sclerosis (*σκληρός*) signifies thickening with condensation."[1] This definition is too limited and vague. The term is strictly descriptive, not of induration from any cause, but of changes in the textural condition of an organ. It involves both quantity and quality. There is overgrowth (proliferation) and transformation of connective tissue, with consequent wasting of the proper functional elements of the part. The processus is constant wherever it happens, and includes both creation, metamorphosis, and destruction. The undue development of the basis-tissue in the liver, kidney, or lung, is always at the cost of the specific material of those organs: so sclerosis of the nervous centres means not only parasitic exuberance of the connective gangue (neuroglia), but proportional compression, deterioration, and annihilation of the ganglion-cells and nerve-tubes. There is histological substitution. This is its univocal characteristic. The perversions of function which we shall study under the name of symptoms are all referrible to it. Comprising what has hitherto been treated of by writers under the name of chronic inflammation of the brain and spinal cord—chronic encephalitis and chronic myelitis,—it is only lately that its true pathogeny has been intelligently set forth. Morbid conditions, hitherto confounded but essentially distinct, have been differentiated, and the proper signs of each modality established.

Sclerosis of the nervous centres is met with in three forms: *A*. Disseminated, Diffuse, or Multilocular; la sclérose en plaques disseminées; *B*. Fascicular, *a*, primitive, *b*, secondary; la sclérose rubannée, primitive ou secondaire; *C*. Cortical, annular, or peripheral; la sclérose corticale ou annulaire.

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[1] Cellular Pathology, Chance's translation, American Reprint, p. 468.

A. DISSEMINATED, DIFFUSE, OR MULTILOCULAR, SCLEROSIS OF THE BRAIN AND SPINAL CORD.

Definition.—*A disease of the cerebro-spinal centres, of gradual invasion; beginning with muscular weakness of one or both lower limbs, subsequently extending to the upper, and, sooner or later, passing into complete paralysis, which may in time affect, in some degree, the muscles of the head, neck, face, pharynx, and tongue; no constant derangement of cutaneous sensibility; the univocal symptom, tremor in the implicated muscles, which happens only when any voluntary movement is attempted, and ceases in a state of rest; frequently nystagmus; attended in the later stages with cramps, and permanent rigidity and contraction of the palsied members; of probably diathetic origin; slowly and surely progressive in its course, and constantly of fatal termination; the anatomical characters being patches or corns of sclerosis, irregularly disseminated, in the brain and spinal cord.*

History.—The first mention of a pathological condition of the nervous centres which was probably sclerosis, is in a report of "*A Palsy occasioned by a Fall, attended with Uncommon Symptoms*," by Dr. A. Maty, dated December 29, 1766, and published in "*Medical Observations and Inquiries*," vol. iii., 2d edition, London, 1769, p. 257. Count Lordat, a French nobleman, had, in the overturning of a carriage, his neck sharply twisted. He quickly apparently recovered from the effects of the accident, and went through a severe military campaign, during which he was constantly exposed to damp and wet. Some time after he noticed an impediment in his speech; then his left arm grew weak, and the corresponding leg; he began to have convulsions all over the body, and became paralytic. The account of the autopsy was sent to Dr. Maty by the count's physician, in France. He writes: "We observed no signs of compression in the lingual and brachial nerves as high as their exit from the basis of the cranium and the vertebræ of the neck; but they appeared to us more *compact than they commonly are, being nearly tendinous.* . . . The central substance *of the brain appeared much browner than usual.* . . . We chiefly took notice of the medulla oblongata; it was *more compact . . . the marrow itself had acquired such solidity as to elude the pressure of our fingers;* it resisted as a callous body, and could not be bruised. This hardness was observed all along the vertebræ of the neck, but lessened by degrees, and was not near so considerable in the vertebræ of the thorax." By the term vertebræ, the region of the spinal cord is evidently meant.

Dr. Marshall Hall (*Diseases and Derangements of the Nervous System*, 1841) relates the case of a man, aged twenty-eight, with tremor of the right arm and leg, who had a peculiar rocking motion of the eyes, and a degree of stammering and defective articulation. This was unquestionably an example of Charcot's disease.

The earliest description of disseminated cerebro-spinal sclerosis is by Cruveilhier, in his *Atlas de l'Anatomie Pathologique* (Paris, 1835–1842). Two cases are given; and their clinical history and morbid anatomy, with the accompanying plates, leave no doubt of their nature.[2] About the same time (1838) Carswell gave an accurate representation of it in his *Illustrations of the Elementary Forms of Disease*, under the head of "Atrophy" (pl. iv., fig. 4). In the descriptive note he says: "Isolated points of the pons varolii, of a yellowish-brown color, patches of the same kind on the spinal chord; all of them occupying the medullary substance, which was very hard, semi-transparent, and atrophied. The atrophy was more conspicuous in some points than in others." In 1856, Ludwig Türck published several cases.[3] Rokitansky mentions the structural change in his Pathological Anatomy.[4] In 1855, Frerichs (Häser's Archiv, Bd. x.) published several cases and described the lesion. The following year (1856), Valentiner, his assistant, made the first attempt at a systematic clinical history of the disorders.[5] He reprinted Frerichs's cases, and collected a number of others (15), which he supposed to be of the same kind. But, according to Dr. Charcot, he did not group them with any exactness, and, under the same title, described conditions quite dissimilar. Hasse, in Virchow's *Handbuch*, and Niemeyer in *Lehrbuch der Special. Patholog. und Therap.*, have followed Valentiner's mixed description. Jaccoud says that Demme published a case of sclerosis of the antero-lateral columns, with microscopical examination, in 1859, but where he does not state, and I have been unable to find it. Prof. Oppolzer, in 1861,[6] published a case of paralysis agitans, with induration of the pons, medulla oblongata, and of the lateral columns, particularly in the lumbar region. In Leyden's well-known contribution on gray degeneration of the cord (1863), the case numbered XXXI. is one of diffuse sclerosis.[7] Frommann reproduced this case in his work on the normal and pathological anatomy of the spinal cord, and details with great minuteness the histology of the lesion.[8] Rindfleisch, too, has described its histology with exactness,[9] and

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[2] Obs. Paraplégie: Degénération grise de la moelle, du bulbe, de la protubérance, des pédoncules cérébelleux, des couches optiques, du corps calleux, de la voûte à trois piliers.—Liv. xxxii., pl. ii., fig. 4.

Obs. Insensibilité presque complète des membres inférieurs. Diminution de la sensibilité des membres supérieurs. Diminution notable, mais relativement moindre, de la myotilité.—Dégéneration grise beaucoup plus considérable dans les cordons postérieurs que dans les cordons antérieurs de la moelle épinière.—Liv. xxxviii., pl. i. and ii. A mixed case, being associated with posterior sclerosis, or locomotor ataxy, and to be more particularly mentioned hereafter.

[3] Türck L. Ueber Degeneration einzelner Rückenmarksstränge. Sitzungsbericht der K. K. Akad. zu Wien; Mathem. Naturw. Classe. 1856.

In previous papers (1851, 1853, and 1855), he had described secondary sclerotic degenerations of the cord. In this he treats of primary sclerosis of the lateral and antero-lateral columns, and gives three cases, with microscopic examinations.

[4] Rokitansky, C. Lehrbuch der Pathol. Anat., Zweiter Band, p. 488, 1856. Ueber Bindegewebs-Wucherung im Nervensystems, Wien, 1857.

[5] Valentiner. Ueber die Sclerose des Gehirns and Rückenmarks. Deutsche Klinik, Bd. xiv., 1856.

[6] Oppolzer. Wiener medicin. Wochenschrift, 36, 38, 1861. Canstatt's Jahrb., iii., 78, 1861. Trousseau's Clinical Medicine, Bazire's translation, vol. i., 446.

[7] Die graue Degeneration der hinteren Rückenmarksstränge, Berlin, 1863. Also Virchow's Archiv. xxix., 202, 1864.

[8] Frommann. Untersuchungen über die normale u. patholog. Anatomie des Rückenmarks, 2 Theil. 77, pl. i., Jena, 1867.

[9] Rindfleisch. Histolog. Detail zu der grauen Degeneration von Hirn u. Rückenmarks. Virchow's Archiv., Bd. xxvi., 1863.

Zenker published (1865) a very carefully-drawn-up case.[10] A case of Skoda (1862) should have been also mentioned.[11] L. Leo's valuable cases were published in 1868.[12]

In France, Drs. Charcot and Vulpian had studied its anatomical characters between 1863 and 1865. On the 25th January, 1865, Charcot read a paper before the Société Médicale des Hôpitaux de Paris, on "Sclerosis of the Lateral Columns of the Spinal Cord, in an Hysterical Woman, whose four limbs had, during life, been permanently contracted." It appeared in the *Gazette Hebdomadaire*, February 17, 1865. The first complete and accurate description of the disease, clinically and anatomically, was, however, by Dr. Vulpian, in 1866.[13] Three cases were given, one of his own, and two of Dr. Charcot's. Dr. C. Bouchard utilized Charcot's and Vulpian's cases, which he had seen at La Salpêtrière, and made a communication on the subject to the Medical Congress at its meeting at Lyons, 1867. In 1866, Dr. Jaccoud gave a clinical lecture at the Hôpital de la Charité on a a case which he styled diffuse sclerosis, and which has been since published.[14] In 1868, Dr. Ordenstein's thesis appeared, in which Charcot's views were represented, and the differential diagnosis between paralysis agitans and disseminated sclerosis was for the first time [15] set forth. In the spring and summer of 1868, Dr. Charcot largely and minutely discussed the subject in his Clinical Lectures at La Salpêtrière. These were subsequently published.[16] A case was communicated to the

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[10] Zenker. Ein Beitrag zur Sclerose des Hirns u. Rückenmarks, Zeitschr. f. rat. Medizin. Bd. xxiv.

[11] Skoda. 'Wien. Med. Halle, iii., 13, 1862. Schmidt's Jahrbucheck, No. 119, p. 294. Syd. Soc. Year Book, 1863, 100.

[12] Leo. L. Deuts., Archiv. f. Klin. Med., 1868, 151.

[13] Vulpian. Note sur la Sclérose en plaques de la moelle épinière. Union Médicale, 1866.

[14] Jaccoud. Leçons de Clinique Médicale, 2d ed., Paris, 1869. In 1 Les Paraplégies et l'Ataxe du Mouvement (p. 245), 1864, he had already alluded to the subject. See also Traité de Pathologie Interne, t. i., 321, Paris, 1869, by the same author.

[15] Ordenstein. Sur la Paralysie agitante et la sclérose en plaques généralisée. Paris, 1867.

[16] Gazette des Hôpitaux, 1868.

Société de Biologie, in January, 1869, by Joffroy. The case of Dr. Pennock, by Drs. Morris and Weir Mitchell, appeared in the *American Journal of the Medical Sciences*, July, 1868. It is the only contribution of this country. It does not appear that the diagnosis was made during life. In 1869, Drs. Bourneville and Guérard published their essay, De la Sclérose en Plaques Disséminées; and subsequently Dr. Bourneville his Nouvelle Étude sur quelques Points de la Sclérose en Plaques Disséminées. They are a reproduction of Charcot's Clinical Lectures, with a collection of all the known cases of the affection. In the last volume of the Memoirs of the Biological Society of Paris, Dr. Liouville has two interesting cases.[17]

To Dr. Charcot, therefore, unquestionably belongs the credit of distinguishing this affection from other paralytic disorders, and notably from paralysis agitans, recognizing its pathological individuality, and tracing its clinical history. He has done for it what Chomel and Louis did for typhoid fever when they established it as a distinct species of continued fever, characterized by a definite group of symptoms.

There has always been much confusion in the clinical history of the disorders of the nervous system, in which tremor is a chief symptom; and the result has been to confound diseases pathogenetically distinct. A proper comparative study of the different forms of tremor, and an accurate investigation of the quality of the symptom have been only recently made. There are two morbid groups in which tremor is a prominent symptom, and in each its character is proper and distinct. In the one it is a constant symptom, or, if temporarily suspended, it is only during sleep; in the other it never happens except as accompanying a voluntary movement; it is always absent when the limb or body is at rest. Galen seems to have noted the two kinds, and to have made a distinction between *τρόμος* (tremor) and *παλμὸς* (tremor coactus). Van Swieten speaks of tremor coactus, always present except during sleep, and tremor debilitate, which accompanies intentional movements. (Comment. Aph., 625.) Dr. Gubler recognized also the two varieties of trembling, but made no attempt at any clinical or pathological interpretation (*Archives Gén. de Méd.*, 5e s., t. xv., p. 702, 1860).

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[17] Liouville. Deux cas de sclérose en îlots multiples et disséminés du cerveau et de la moelle épinière. Comptes rendus des séances et Mémoires de la Société de Biologie, t. xx., 1869.

All the English authors confound this disorder with paralysis agitans. Parkinson, whose description of shaking palsy has been closely followed, unquestionably did. He was aware of Maty's case, refers to it, and expresses the opinion that the probable morbid condition in paralysis agitans may prove to be induration of the upper part of the medulla spinalis, oblongata, and pons, due, as he supposes, to simple inflammation, or rheumatic, or scrofulous affection of the nervous substance or membranes. Dr. W. R. Saunders (Reynolds's "System of Medicine," vol. ii.), in an excellent article on paralysis agitans, confuses it with diffuse cerebro-spinal sclerosis; though aware of the sclerotic lesion of the cord, he is greatly at fault when he attempts to connect it with its proper clinical history. For example, he says: "In more inveterate, especially senile cases, paralysis agitans appears to depend on a discoverable lesion, namely, an atrophic condition of the spinal cord," etc. (p. 199). Again: "This atrophy would explain the chief features of the disease. . . . the occurrence in old age, . . . under conditions of premature senility" (p. 199). Previously (p. 186) he writes: "Lastly, the term paralysis agitans, or shaking palsy, has been applied to cases of ordinary motor paralysis (hemi- and paraplegia) complicated with tremors—a complication not uncommon in diseases of the brain, and in certain cases of chronic myelitis, and of locomotor ataxia. . . . Parkinson's malady is *idiopathic* paralysis agitans, in which the tremors are the chief and earliest symptom, and the paralysis entirely subordinate and peculiar, true hemi- or paraplegia being rare complications; while, in the cerebral and spinal affections just referred to, the loss of motion or sensation is the main feature of the disease, and the tremors and spasmodic agitations are only concomitants (i. e., the paralysis agitans is *symptomatic*). Hence the latter kind of cases should be styled, not paralysis agitans, but hemi- or paraplegia, or spinal or cerebral disease *complicated with paralysis agitans;* i. e., with spasmodic tremors. This description, which is essential for the accurate definition of Parkinson's disease, has often been overlooked, and requires, therefore, to be specially insisted on." Really, though imperfectly, describing diffuse sclerosis of the nervous centres, at p. 193, he says: "Occurring in middle life (twenty-five to fifty), however formidable in appearance, it is susceptible of amelioration, and sometimes of cure." There is other evidence in the article to show how unsatisfactory the writer's notions are respecting the disease we are about to describe.

Clinical History.—Although the anatomical characters of this affection are constant, and always of the same histological constitution, their territorial distribution may vary. This capriciousness of site necessarily qualifies the symptoms, and it is necessary to admit several forms of the disorder, each to a certain extent represented by proper functional disturbances; these are determined by the exclusive, or predominant, occupation by the lesion of one or more districts of the cerebro-spinal system.

Disseminated sclerosis of the nervous centres may be described, therefore, under three divisions, according to situation: *a*, cerebral form; *b*, spinal form; *c*, cerebro-spinal form.

a. Cerebral Form.—It is doubtful if this form ever happens strictly alone. The only recorded observation is that by Valentiner and Frerichs, and here the spinal cord was not examined after death with proper care.

Case.—A youth, nineteen years of age, was suddenly seized, without apparent cause, with unilateral (left) motor and sensory paralysis, which soon extended to the other side. There was general muscular unsteadiness, and voluntary movements brought on excessive tremor in the extremities, which after a while was induced by any attempt at speaking or by moral emotion. The gait was oscillating, and the patient obliged to use crutches. At first, some mental excitement was noticed, but there soon succeeded spells of melancholy. There were eccentric neuralgia and twitchings in the affected muscles, and subsequently attacks of vertigo, with pain in the occipital region. Two years after the outset, paralysis of the limbs was almost complete, though still most marked on the left side; the tremor had increased and was nearly constant; speech unintelligible; urination and defecation irregular; nystagmus; and gradual weakening of the intellect. A few days before death, which occurred about this time, tremor ceased. Patches of sclerosis were found in the pons varolii, olivary bodies, and at the base of the brain.

The latest British authority, Dr. Handfield Jones (Studies on Functional Nervous Disorders, 2d edition, London, 1870), treating of paralysis agitans, remarks: "It appears to me a question whether two distinct affections are not often comprehended under this name" (p. 383). But, from what immediately follows, it is clear that he had not diffuse cerebro-spinal sclerosis in mind, as one of the two indiscriminated affections. He goes on to say: "For, on the one hand, it appears pretty certain that there is one form which is *met with in old persons, is quite incurable*, and is *associ-*

ated with, if not *dependent on*, *organic wasting changes in the nervous centres;* while another form occurs in *younger persons*, is more curable, and is therefore presumably not dependent on organic change."

b. Spinal Form.—The invasion is usually very gradual, the first symptoms being tingling and numbness in one or both legs, or soles of the feet, which are soon followed by weakness in the limbs; this last may, however, be an initial symptom. The paresis grows worse by degrees, and if it has been limited to one extremity, as is often the case, will, after a while, involve the other, and extend to the upper extremities. The gait, which from the outset may have been more or less unsteady, is now staggering like that of a drunken man. All muscular acts are uncertain, and rhythmical spasms accompany any voluntary movement of the affected muscles. Cutaneous sensibility, in respect to touch, pain, and temperature, is unaffected, the cases in which, at this time, any modification has been noticed, being exceptional. The general health is good. As the disease advances, paralysis, more or less complete, succeeds the paresis; all the symptoms just described worsen, and new ones appear. They are: 1. Tonic muscular spasms, occurring spontaneously, or after artificial excitation. Although generally happening at a late period, they have been met with during the early stages, particularly in those cases in which paraplegia has set in soon. These cramps are mostly limited to the lower extremities, rarely affecting the upper, and, when they do, only in a slight degree. One or both lower limbs may become suddenly rigid, and any attempt to move them in certain directions is found to be difficult or impossible. After some time this rigidity passes off, and the affected member may be moved at the will of the operator. 2. Permanent contractions, which follow strictly in their development the course of the palsy, striking first the legs, then the arms, and occasionally the muscles of the trunk. The position of the lower extremities is, for a while, that of fixed extension, while the fingers are bent inwards. Finally, motility, which has been progressively growing weaker, is totally abolished; all power of voluntary motion in both the upper and lower extremities is lost, and the unfortunate patient is condemned to keep his bed. The legs are forcibly flexed on the thighs, the thighs on the pelvis, and the heels are drawn closely up towards the buttocks. It is almost impossible to straighten the limbs, an effort to do so causing great pain. Sensibility is still often intact. In some patients, reflex movements may be provoked by pinching the skin, or tickling the soles of the feet, while in others these excitations are without result. Finally, the general health fails; nutrition becomes defective, and there is rapid emaciation; sloughs form on the sacrum, and death happens from exhaustion, or some intercurrent acute disease.

There are several varieties of this form of spinal sclerosis, as one or other column of the cord is chiefly or exclusively affected; or more than one column may be implicated, and in such cases the symptoms will be of a mixed character, as when the lesion involves the posterior and anterior, or antero-lateral, columns at the same time. In such cases the phenomena proper to locomotor-ataxy will be associated with those of the spinal form of disseminated sclerosis. (See Cases in Appendix.)

c. Cerebro-spinal Form.—The accession may be insidious, as in the preceding form, or abrupt. Simultaneously with, or preceding, or following the motorial troubles, there are ocular or cerebral disorders, as constant vertigo, pain in the head, difficult articulation, weakness of sight, or diplopy; these may be only transitory. In some instances there have been occasional attacks of cerebral congestion, without loss of consciousness, but followed by temporary hemiplegia (Charcot, L. Leo). Sooner or later tremor affects the extremities and eyeball (nystagmus). The paralytic symptoms follow very much the same course as in the spinal form, attended with rigidity and permanent contraction. These latter have appeared as early as two years after invasion, but most generally not sooner than five or six years, so that if the patient should die from an acute disease before that time they may be altogether wanting. The spasmodic jerkings of the paralyzed muscles, spontaneous or induced, are nearly always limited to the inferior extremities. The ocular troubles get worse, and the ophthalmoscope shows slight dilatation of the retinal veins, and some atrophy of the papilla. Articulation is more difficult and fragmentary; the words are as it were scanned, each syllable being distinctly and slowly pronounced, owing to muscular weakness hindering the movements necessary to proper utterance, and not to any cerebral defect. The several forms of cutaneous sensibility in many cases are undisturbed, or nearly so. The expression of the face is natural. The mental faculties may be for some time perfect, but after a while memory is impaired, the temper becomes irritable or melancholic, and intelligence grows feebler by degrees, until it is quite lost. The general functions are yet good, though there may be constipation and frequent micturition. The progressive loss of motility renders the patient perfectly helpless; the muscles of the mouth and pharynx are paralyzed, and mastication and the deglutition of solids, and even of liquids, difficult or impossible; the saliva accumulates in the mouth, or dribbles out of the corners of the lips, after ineffectual attempts at swallowing it. Sloughs form on the parts of the body exposed to pressure. Rapid deterioration of the system causes death from exhaustion; or an attack of caseous pneumonia, bronchitis, erysipelas, dysentery, or acute phthisis, or apoplexy, proves quickly fatal.

Consideration of the Special Symptoms.—*Invasion.*—In a few cases, as has been stated, the onset is without warning or apparent cause, the patient becoming suddenly paraplegic—in one instance with lessened sensibility. Or the attack may begin with an apoplectic seizure with or without loss of consciousness, and followed by temporary hemiplegia, and fits of vertigo. But in a large majority the approach is insidious, and before any weakness in the lower extremities is complained of, there is tingling in the legs and soles of the feet, with occasional numbness and coldness, and a sense of fatigue after slight exertion. Some stiffness and awkwardness in the movements may be noticed; and in a few instances, probably where the posterior columns of the cord were affected from the outset, there have been darting pain-spells.

Motility.—The paresis may be regularly progressive from the outset, or it may abate from time to time. After a while the gait becomes uncertain; the patient stumbles on level ground, or on meeting with the slightest obstacle; he gets weary quickly,and is obliged to use a cane, or some support. The loss of voluntary power in the muscles rapidly increases, and the limbs are useless. The paralysis follows invariably the course of the paresis; and frequently in this order: first, the left leg, then the right, the left upper extremity, next the right; afterward the muscles of the face, neck, and trunk.

Tremor.—This is the univocal pathognomonic symptom, to which all others are secondary, and by which the diagnosis is made. It gives the disease its special physiognomy, like the peculiar gait of locomotor ataxy. The period of its appearance is variable, and often difficult to fix. In one case it was evident three months after the onset; in one, eight months; in one, fifteen months; and in others it was not noticed until several years after the paraplegia had set in. It never occurs except on some voluntary muscular effort, or, as happens in the later stages, when excited by mental emotion, as the sight of a stranger, or the examination of the physician. At first it is slight; and, if the patient keeps his bed, may be overlooked, for it is always absent when the muscles are at rest. Under these circumstances it must be sought for; the patient should be made to execute certain movements, as carrying his hand to his head, or a vessel to the mouth. In the erect position a series of oscillations takes place, the equilibrium being maintained with difficulty, and the body swaying towards all sides. If walking is possible, titubation or staggering, as in the gait of a drunken man, is well marked, and there is a constant tendency to fall over. Like the paralysis, tremor extends upwards, and after the arms and hands, the head, eyes, and tongue, may be affected. An attempt to drink will set the head shaking, or even raising it from the pillow. The oscillatory movements of the eyeball (nystagmus) are often so great as to hinder ophthalmoscopic exploration. They usually occur only on using the eye, and trying to fix an object. The nystagmus is nearly always binocular, only one case being reported where it was monocular. When the tongue is protruded it is found to be tremulous; words are uttered in a peculiar manner, the difficulty of articulation increases, and, finally, speech is unintelligible.

The kind or type of the tremor, is a series of rhythmical twitchings (secousses) or short spasms, which at first are moderate, and do not prevent the usual movements of the affected limb, but modify their mechanism. In raising a glass of water to the lips, the gesture is not continuous and harmonious, but broken, jerking, and ill-regulated; the control of the will over the muscular effort is manifestly weakened; and the hand is carried in various directions before the act is accomplished; or, from the violence of the jactitations, it may become impossible. The tremors of the limbs are usually in the direction of flexion and extension, sometimes of ab- and adduction, and occasionally in that of rotation. The head nods, rolls, or partially rotates; and the motions of the jaw are lateral.

Towards the end, when the limbs are permanently contracted, and consequently immovable, the tremors cease in them, but may continue in the head, neck, and trunk.

Sensibility.—Sensibility to touch, pain, tickling, and temperature, may be not at all or but little deranged. Partial hyperæsthesia was noticed in a few instances. In two cases there was muscular anæsthesia, the notion of position of the limbs being lost, but cutaneous sensibility was natural (L. Leo). One patient had analgesia (Charcot); and in another contactile discrimination was lessened or wanting in certain areas of the skin (Liouville). A superficial burning heat in the lower extremities was constantly felt by one individual; and Dr. Pennock had the sensation of a narrow band around one of his legs.

Of the special senses, vision is the one most commonly affected. The sight is frequently weak from the outset; photopsia has occurred with amblyopia. Once it is stated that the sense of smell was lost, and at the autopsy patches of sclerosis were found in the olfactory nerves.

Contractions.—These never occur before the paralysis is fully established, usually after several years, and hence belong to a late period of the disease. They too follow the line of attack of the palsy; beginning in the lower extremities, they are at first partial and intermittent, but finally become generalized and permanent, often extending to the muscles of the jaws and trunk, in which case the patient lies a helpless, inert mass, drawn up in a heap. The direct relation between symptom and lesion has several times been demonstrated. In one case of contraction of the left arm, examination of the cord after death showed that the two anterior columns, and the left lateral column, at the level of the middle of the cervical enlargement, had undergone sclerotic transformation (Charcot). In another, where there had been rigidity of the right upper extremity with slight flexion of the forearm and permanent extension of the extremities, a band of sclerosis on the right antero-lateral column extended from just below the olivary body to the upper part of the cervical enlargement.

Cramps.—These occur in paroxysms, lasting from a few minutes to several days. A limb may of a sudden become thoroughly rigid, and resist all efforts to move it. One instance is mentioned where the lower extremities were seized when in a state of adduction. They are often very painful, and when over leave a sense of fatigue in the limb.

Muscular spasm-spells, the spinal epilepsy of Brown-Séquard, are almost limited to the lower extremities. The limbs are jerked about as by rapid electric shocks. When their degree is less, the spasms are like the muscular startings from strychnine. If they happen before motility is completely abolished, they may alternate with the cramps.

Patho-Anatomy.—The morbid appearances of the *spinal cord* will be first described. The membranes are usually healthy, although sometimes there are a few brown or amber-yellow stains on the pia mater immediately over the affected segments of the cord; the stellate cells in these spots are filled with pigment matter.[18] Often, through the pia mater, *grayish patches* may be distinguished here and there on the surface of the cord. On stripping off the membranes, which is easily done, these are found to have irregularly oval but, well-defined outlines, of an ashen or ground-glass color, and to be distributed over several columns of the same side, or confined, or nearly so, to symmetrical columns. They frequently intersect the fissures and lines of emergence of the nerves. Their dimensions are variable, generally from three to four centimetres long, by two or three broad; they may be mere grains or linear streaks. They are firmer to the touch than the surrounding tissue, and somewhat depressed and shrunken, though in a few instances they were slightly prominent (Cruveilhier, Charcot), probably indicating an early stage of development, for they were turgescent and less dense. By exposure to the air the patches acquire a rosy or salmon tint.[19] On slicing them, they prove to be conical masses, more or less deeply embedded, or rather wedged, into the white substance. They shade off imperceptibly into the healthy tissue, there being no precise delimitation. (Fig. 1.) Most commonly discrete, they are sometimes confluent, and Cruveilhier has represented this arrangement very accurately in one of his plates.

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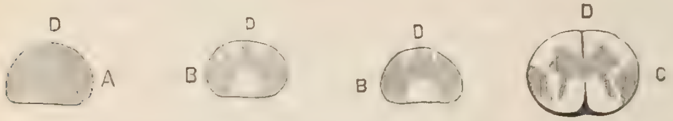
[18] In one of Vulpian's cases (No. 2) there were several fibroid patches adherent to either layer of the arachnoid. There was hyperæmia of the pia mater of the inferior segment of the cord in another (No. 3), with some cartilaginous patches on the visceral layer of the arachnoid. When meningitis is found after death, it is almost always recent, about the cauda equina, and ascending, and secondary, being caused by the sloughs on the sacrum.

FIG. 1.



Transverse sections of the cord. D, Anterior fissure. A, Above cervical enlargement. B, B′, In the middle portion of cord. C, Three centimetres above terminal end of cord. —CHARCOT.

In the medulla oblongata the patches are met with separately or conjointly on the olivary and restiform bodies, and the pyramids, but most often on the olivary bodies. The pons varolii is the frequent seat of sclerosis, especially its inferior surface, where it appears as gray patches, with a wavy contour, disposed transversely across the median line, and may extend to the cerebral peduncles in one direction, and the medulla oblongata in the other. In one case, the aqueduct of Sylvius was surrounded by a large patch, with processes on the pons, and towards the fourth ventricle. (Fig. 2.)

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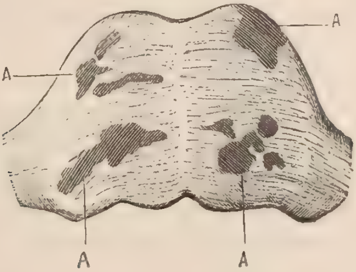
[19] If portions of the cord are placed in a solution of chromic acid, the diseased tissue becomes at first yellow, then white and opaque, contrasting strikingly with the greenish-gray tint of the healthy substance; or, as suggested by Bouchard, a camel's-hair pencil, moistened with an ammoniacal solution of carmine, may be passed several times over the surface, which is then exposed to a stream of water. The carmine will have stained only the altered tissue.

Fig. 2.



Section of the pons varolii. A, A, A, A, Patches of sclerosis on cut surface of upper portion.—Charcot.

The cortex of the cerebellum is scarcely ever affected, the patches of sclerosis, when present in the organ, being in the white substance, or in the corpus dentatum.

Rare in the convolutions, the patches sometimes are found where the gray and white substances join, but are more common on the walls of the lateral ventricles, and may extend to the intraventricular nuclei of the corpora striata. When existing in the centrum ovale, corpus callosum, and septum lucidum, they are very apparent, and are generalized. They have occurred in the optic thalami.

Though as a rule the cerebral nerves escape, in one of Cruveilhier's cases, the hypoglossal, glosso-pharyngeal, and pneumogastric were implicated; and in two cases reported by Drs. Vulpian and Liouville, the optic and olfactory nerves had patches visible to the naked eye; and in one of Dr. Ordenstein's observations the hyperglossal and left motor oculi externus were distinctly sclerosed. In their configuration they resembled the patches in the pons and medulla oblongata, and in nowise could be confounded with the tissue-changes of these nerves noticed in locomotor ataxy.

With one exception, the spinal nerves have been free from sclerosis, the nerve-roots often issuing perfectly sound from the midst of a patch. At a meeting of the Société de

Biologie, July 30, 1869, Dr. Liouville stated that he had seen one case where the spinal nerves had sclerotic patches (Mémoires, etc. l. c.)

Histological Alterations and Microscopical Appearances in Different Stages.—In studying in detail the essential anatomical characteristics of this lesion, it is necessary to understand the modifications the histological elements undergo in the successive phases of the morbid processus. The minute anatomy of the nervous system has been accurately studied but quite recently, and though there are yet many points under discussion, certain fundamental facts have been generally accepted; these should be borne in mind to intelligently appreciate the exact nature of the tissue-changes in disease. The proper elementary structure of the nervous centres, consisting of the nerve-fibres or tubes, and ganglion-cells or corpuscles, lies embedded in, or, more correctly, is bound together by, a kind of connective tissue, which Kölliker called reticulum, and Virchow neuroglia. It is this connective framework, or gangue, which is specially interested in the lesion under consideration.[20]

If a thin section of the cord, hardened in a solution of chromic acid, and colored by carmine, and afterwards made transparent, be examined with a lower power, the white matter seems at first to be entirely composed of small, regularly-rounded bodies, or disks, placed side by side, and of about the same diameter; these are the cut-nerve tubes; the small red points, looking like minute globules, in the midst of these shining translucent disks, are the colored axis-cylinders. A closer examination shows that these disks are not contiguous, but are really separated by an apparently homogeneous substance, not so deeply tinted as the axis-cylinders, and which seems to fill up, like a cement, the interspaces between the nerve-tubes. This is the neuroglia or reticulum. In the gray substance this interstitial matter is still more abundant than in the white; certain portions are wholly formed by it, as the periphery of the centrum ovale, and the central thread of the ependyma (Virchow). It is in excess, too, in the gelatinous substance around the expanded extremity of the posterior cornua of the cord, and in the posterior commissure, which, when treated with a solution of carmine, shows a nearly uniform reddish hue, while the anterior commissure, from the number of nerve-tubes which cross it transversely, is less perfectly stained. The meshes of the neuroglia are finer in the gray substance than in the white, and its texture appears more cellular or spongy. This connective framework in both substances gives support to the ramifying blood-vessels. The zone of this tissue around the peripheral portions of the nervous centres, described by Bidder and Frommann, is called the cortical layer of the reticulum; it sends processes towards the central parts of the cord, which form triangular compartments or trabeculæ, their bases being on the circumference, and apices in the gray matter. These processes divide and subdivide, forming a reticulated arrangement, whose interspaces vary in size; in the largest of these, eight or ten nerve-tubes may be lodged, while the smaller ones may contain only a single tube.

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[20] The connective tissue of the cord was first investigated by Küffel (1810). Cruveilhier, in the article Apoplexie, Dictionnaire de Médecine et de Chirurgie Pratiques, ed. 1820, writes: "The extremely delicate serous cellular tissue which unites and separates the cerebral fibres, and which forms an excessively fine web. . . . ."

What is the histological constitution of the neuroglia? This is an open question, and micrographers are still disputing about its intimate nature. On one point they are, however, pretty generally agreed, namely, that it is not ordinary connective tissue.[21] The opinion most largely held (Frommann, Max Schultze, Kölliker), and which Dr. Charcot's personal observations tend to confirm, is, that it is constituted as the stroma of the lymphatic glands, after the type of the simple reticulated connective tissue of Kölliker. If, according to those authorities, thin sections of the cord, prepared in chromic acid, and colored with carmine, are examined under the microscope, the following conditions are noticed: 1. In the white substance, at those points of the reticulum where several trabeculæ meet, enlargements or knots are seen, in whose centre are nuclei, with a well-defined contour, deeply colored, and which are the myelocites of Robin, and the nuclei of the neuroglia of Virchow.[22] In transverse sections, the trabecular structure, with its homogeneous, brilliant, fibroid-looking walls, constantly anastomosing, seems to form interspaces con-

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[21] The anatomical fact of the reticulum or neuroglia, is disputed by several histologists, especially by Robin and Henle. The former asserts that it has no natural existence, but is a product of art. He holds that in a fresh state, before the cord has been subjected to the action of reagents, the spaces between the nerve-tubes are not filled by a reticulated connective tissue, but by a grayish, soft, amorphous, and finely granular matter, in the midst of which the myelocytes (the nuclei of the neuroglia of Virchow) are suspended. Alcohol and certain acids, especially chromic, cause this matter to become hard without shrinking, and it is to this property that the reticulated arrangement is due.* The answer to this is: while it is admitted that in the fresh cord amorphous matter in very

* Programme du Cours d'Histologie, 1870. Dictionnaire Encyclopédique des Sciences Médicales, 2me serie, t. i.

small quantity is interposed between the nerve-elements, and that in this state the reticulum is less clearly made out than in sections which have been hardened in chromic acid, it is not less true that in fresh pieces of the cord the white substance, placed in iodized serum, and dilacerated under the microscope, show unquestionably on their edges the presence of connective tissue (Kölliker, Frommann, M. Schultze); and that this tissue is more susceptible of demonstration in certain morbid conditions, in which the healthy structure becomes exaggerated, without undergoing complete transformation (Virchow). This is particularly the case in simple sclerosis, when the alteration has not gone beyond the first stage of its evolution; the effect of these reagents is limited to rendering more distinct the reticulated structure of the connective framework of the spinal cord.† Henle and Meckel have recently attempted to show, by observations which are entitled to consideration, that the chemical reactions of the neuroglia are directly the opposite of those of connective tissue. (*Ueber die sogenannte Bindessubstanz der Centralorgane des Nervensystems*, Henle and Pfeufer's Zeitschrift. Bd. xxxiv., 1869). Of course, the pathogenetic processus of sclerosis of the nervous centres is differently interpreted according to the histological theory held: one set regarding it as an exaggerated development of preformed tissue—a simple hyperplasia, as sclerosis of the lung, liver, spleen, etc.; and the other, as a lesion *sui generis*, consisting of the heterologous, or abnormal formation, of an element or tissue, differing from the physiological type of the organ; in fact, a ***heterotopical neoplasm.***

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[22] Virchow, R. Die krankhaften Geschwülste, t. ii., p. 127; 1864–'65.

† Hayem. Différentes Formes de l'Encéphalite, 1868. Hayem and Magnan. Journal de la Physiologie, No. 1, 1869. Deiter. Untersuchungen über Gehirn und Rückenmarks, p. ii., Braunsweig, 1865.

taining the nerve-tubes; while in longitudinal sections they appear to subdivide and ramify indefinitely, producing the finest meshes, which are interposed between the nerve-tubes. The interspaces between the sheaths and the tubules are filled by a small quantity of finely-granular amorphous matter.

We will now proceed to briefly examine the histological changes which are met with in those portions of the spinal cord affected with disseminated sclerosis.

According to Frommann, who had an opportunity to observe the tissue-changes at an early stage of the morbid processus, and Rindfleisch, the walls of the arterioles and capillaries are thickened by a proliferation of small, rounded cells; the cells of the neuroglia are increased in size, and their processes dilated; the nuclei are large and surrounded by a layer of protoplasm, while their number is increased, one cell containing from two to ten nuclei, either in one mass, or each with its own membrane, in which case each of these nuclei appears as a cell detached from the envelope and contents of the primitive connective corpuscle. In consequence of the progress of the nuclear multiplication, nuclei form in the processes of the connective corpuscles, and these processes grow larger as their anastomoses become more apparent. A little later the anastomotic net-work of the connective processes is filled with nuclei, and it is by the abnormal accumulation of these elements that the sites of the original cells are discerned.

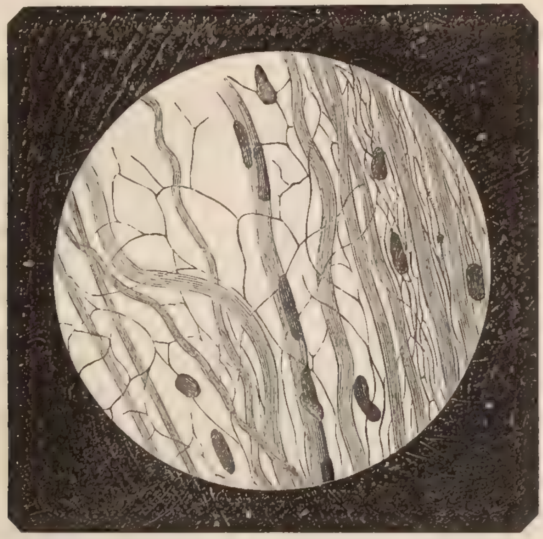
The sum of these changes may be shortly stated: as an overgrowth of connective nuclei and cells, and the subsequent development of fibrillary elements of great tenuity, which may be seen on the cut surfaces, and behave like elastic tissue. These elements originate probably in the amorphous fundamental matter of the neuroglia. According to Rindfleisch, the morbid tissue is moistened by a viscous, intercellular liquid, slightly coagulable in water, and containing the nuclei and small uninuclear cells; by pressure, this liquid is forced out on the surface of a recent section, giving rise to the ependymoid formation of Rokitansky.

In consequence of the modifications of the reticulum, the nerve-elements are compressed and atrophied; the tubes lose their myelin, which undergoes granular disintegration; the axis-cylinders become paler and very translucent, with, frequently, a longitudinal striation, or have a finely-granular aspect, which Frommann regards as the beginning of the molecular destruction. Around the atrophied nerve-tubes a large number of globules are seen, formed by a coagulated fatty matter. In proportion as the atrophy of the nerve-elements advances, the fibrillary tissue is developed, and gradually assumes the characters of genuine connective tissue. Regressive transformations now take place, fatty granulations are deposited in the interstices of the tubes, and amyloid corpuscles are produced at the expense of the small uninuclear cells of the intercellular viscous liquid. The absorption of this liquid, favored by the contraction of the connective elements, marks the last stage of the processus. The diseased tissue, deprived of its juice, is composed solely of connective fibres immediately contiguous, whose unequal shrinkage gives rise to partial distortions. Finally the whole mass becomes of a grayish fawn-color, often closely resembling the tint of ground glass.

Charcot has described these histological changes in the several stages of the processus more fully. On placing under the microscope a properly-prepared thin transverse section of the diseased cord, several concentric zones are recognized; these correspond to the different phases of the lesion. *a.* In *the peripheral zone*, the trabeculæ of the reticulum are thickened; the nuclei of the enlargements of the reticulum are swollen; the nerve-tubes are atrophied at the expense of the medullary sheath, the axis-cylinder being whole. *b.* In *the second, or transitory zone*, the size of the nerve-tubes is still less—indeed, in many the myelin has disappeared, the axis-cylinder only remaining, which may be hypertrophied. The trabeculæ are more transparent, their contour less definite, and at certain points they are replaced by bundles of long, delicate fibrillæ, analogous to those which are characteristic of common connective tissue. These are developed at the expense of the meshes which contain the nerve-tubes, and the consequence is that the reticulated aspect of the healthy neuroglia has a tendency to disappear. *c.* In *the central zone*, that is, in the midst of the sclerosed patch, all traces of fibroid reticulum have gone; there are neither cells nor trabeculæ; bundles of fibrils fill the alveolar interspaces, from which the myelin has entirely disappeared; the axis-cylinders are atrophied to such a degree that it is hard to distinguish them from the newly-formed fibrils. The persistence of these cylinders in the midst of the tissue which has undergone fibroid substitution, is, Dr. Charcot thinks, peculiar to disseminated sclerosis.

The characters of the fibrillary tissue are particularly well exhibited in longitudinal sections. It is seen to be composed of parallel fasciculi, formed of delicate, opaque, smooth fibrils, rarely subdividing and anastomosing, but frequently interlacing, after the manner of felting, and which are colored by carmine. These characters enable us to distinguish it from the axis-cylinders, which are usually larger, transparent, and do not ramify. (Fig. 3.)

FIG. 3.



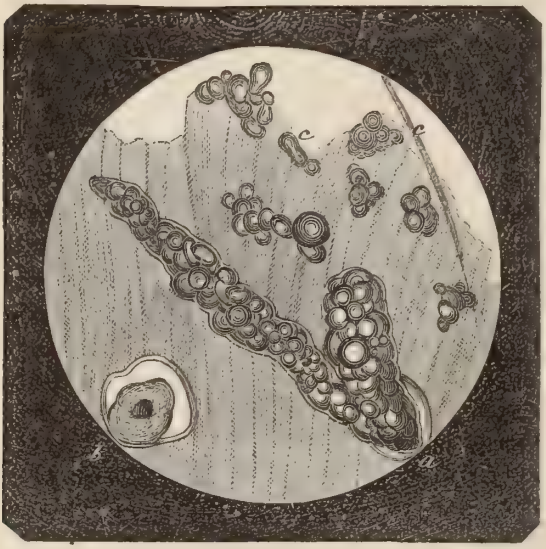
Recent preparation from centre of a patch of sclerosis of the cord, colored by carmine and dilacerated. In the centre a capillary vessel with numerous nuclei. On either side axis-cylinders, some large, others quite small, deprived of their myelin. The capillary and the axis-cylinders, strongly colored by carmine. The latter are perfectly smooth, and have no ramifications. Between the axis-cylinders, delicate fibrils of recent formation are seen; on the right nearly parallel to each other, while in the centre and on the left, they form a sort of net-work, resulting from their interlacement, or anastomosis. They may be distinguished from the axis-cylinders by their diameter, which is much less, by their numerous ramifications, and by not being colored by carmine. Scattered nuclei are also visible; several are in connection with the fibrils, and others are of irregular form, from the action of the ammoniacal solution of carmine.—CHARCOT.

They may be distinguished, also, from the fibres of the reticulum by these being thicker, shorter, and always having branchlike processes. Finally, they differ from the elastic fibres by their swelling up from the action of acetic acid, and forming a transparent hyaline mass, which is not the case with the elastic fibres. In the midst of this fibrillary tissue there is always a considerable number of corpora amylacea.

The vessels which traverse the patches of sclerosis, undergo alteration. In the peripheral zone, the walls of these vessels are thickened, and contain more nuclei than in the normal state; nearer the centre the nuclei are more numerous, and the lymphatic sheath is replaced by several layers of fibrils like those developed in the thickness of the reticulum. Owing to the concentric hypertrophy of their walls, the calibre of the blood-vessels is lessened.

There are certain changes which can be best or only studied in the recent state. In the centre of the sclerosed mass, globules or granules of a fatty kind are nearly constantly met with. These elements have two chief aspects: in the one the masses have sinuous, dark outlines, with, like the myelin, a double contour; the other are true fatty granulations, sometimes isolated, at others agglomerated, so as to form granular bodies. (Fig. 4.)

Fig. 4.



Patch of sclerosis from cord in a recent state. ***a.*** **Lymphatic sheath of a vessel distended by large fatty globules.** ***b.*** **Vessel cut across; the lymphatic sheath is separated from the tunica adventitia by an empty space, the fatty globules which distended the sheath having disappeared.** ***c. c.*** **Little collections of fatty globules scattered over the preparation.—Charcot.**

These drops of myelin and fat granules may infiltrate through the meshes of the reticulum, and spread themselves abroad; they never occupy the centre of the patch, for there the fibrillary metamorphosis and the destructive process as regards the nerve-tubes are ended; but they are seen about its periphery at those points where the medullary sheath has almost disappeared, by the compression first of the trabeculæ, and afterwards of the fibrillary fasciculi, which gradually invade and occupy the alveolæ. The nerve-tube is finally represented by the axis-cylinder alone. The accumulation of the medullary or fatty globules and the destruction of the medullary sheath are contemporaneous, and when the one is accomplished the other ceases to occur. We may then infer that the medullary and fatty corpuscles are detritus resulting from the disintegration of the nerve-tubes. Dr. Charcot believes that the absence of these fatty granules in the centre of the patches is due to their absorption, and this view seems sustained by the fact that within the lymphatic sheaths of the arterioles those fat-corpuscles are sometimes so abundant as to notably increase their volume, and are visible by the naked eye in the form of white lines on the gray ground of the patch. The changes in the vessels before described are not a primitive lesion, or an essential part of the degenerative processus, but simply a consecutive fatty infiltration of the lymphatic sheaths.

These morbid changes are also met with in the gray matter; the ganglion-cells, however, are not the seat of the nuclear proliferation observed in the connective cells, but undergo a peculiar alteration known by the name of *yellow degeneration*, from its ochrey tint: they can no longer be colored by carmine, as in the healthy state; and finally, all the constituents of the cell become atrophied.

The consecutive phases of the morbid processus may be summarized as consisting: 1. Of the initial fundamental fact —*proliferation of the nuclei, with concomitant hyperplasia of the reticulated fibres of the neuroglia;* and, 2. Of the *secondary, consecutive atrophic degeneration of the proper nerve-elements.*

Differential Diagnosis.—This affection has sufficient individuality to secure, in most instances, at least, its ready recognition, notwithstanding resemblances between its several forms and other diseases of the nervous system. The disorder with which it has been heretofore invariably confounded is, as has been stated, paralysis agitans, or shaking palsy. Besides a pathogenetic distinction founded upon the sure basis of morbid anatomy, there are differences of clinical history which should forbid confusion or mistake. Paralysis agitans is chiefly met with in persons of declining years (Trousseau). Sauvages says, "While chorea, or *scelotyrbe sancti viti* attacks the young, ballismus, or *scelotyrbe festinans*, attacks those of advanced life;" and Saunders observes, "Age is of primary importance, both in causing the affection and aggravating it." Diffuse sclerosis of the cerebro-spinal centres is a disease of adult life. There is some parallelism in the invasive stage of the two disorders, the approach of both being often so imperceptible that it is difficult for the patient to fix the exact period of commencement. In both there are crawling sensations and numbness, and sense of diminution of muscular power; but in the neurosis these are almost constantly felt in the arms, while in the organic disease they are almost as constantly experienced in the legs. In paralysis agitans, a tendency to trembling is an initial symptom, and precedent to the paresis; in sclerosis, on the contrary, tremor invariably follows paralysis, limb by limb. The muscular weakness of paralysis agitans begins in one arm, or both, and then extends to the lower extremities, and when it does pass into true paralysis, which is very rare, it is in the final stage; in diffuse cerebro-spinal sclerosis, one or both lower limbs are first attacked with paresis, and, sooner or later, become completely deprived of all motor power, rendering the patient perfectly paraplegic and unable to walk. Hence the gait in the two diseases is eminently diacritic. A patient with shaking palsy in its developed stage, after the customary balancings and oscillations of the body, starts with the head and trunk bent forward, on the toes and forepart of the feet, with short, quick steps, and, to maintain the centre of gravity, thus displaced, goes trotting and hopping along, at almost running speed, with one or both arms and wrists semiflexed and closely pressed to the sides; he seems, as Trousseau observes, to be running after himself. This festinating, procursive gait is pathognomonic. Nothing of the kind is seen in cerebro-spinal sclerosis; the gait is that of paraplegia, according with the degree of palsy, and will be presently more fully described. *Tremor* is a prominent and common symptom of the two affections, and from this has arisen much of the confusion which led writers so persistingly to associate pathological species genetically distinct; for when this sign is analyzed and comparatively studied in the two disorders, great differences, not only of development but of degree, become evident. One of the earliest phenomena of shaking palsy, in diffuse sclerosis it is invariably consecutive to the motorial troubles. In the former disorder, trembling is incessant, and is but little modified whether the patient be at rest or in motion, and in the developed stage is scarcely interrupted by sleep; while in the latter, it is never spontaneous, but is always provoked by, or follows upon, any muscular movement; and though it may be started by emotion, it will be found, on observation, that this deviation from the rule is only apparent, for some change of position of a limb, or of the head or trunk, is always involved. Nystagmus, so constantly present in sclerosis, is never met with in paralysis agitans. The peculiar slow, scanning articulation of a sclerotic is very different from the embarrassed, indistinct utterance noticed in paralysis agitans. The intellect in paralysis agitans is generally unaffected until near the close, while in diffuse cerebro-spinal sclerosis it is mostly weakened from an early period. The characteristic deformities, described first by Parkinson, and recently by Charcot,[23] which occur in shaking palsy, cannot be confounded with the permanent contractions of late muscular rigidity, so constant in the terminal period of diffuse sclerosis.

There are some symptoms in common between the ordinary forms of multilocular sclerosis of the anterior, or antero-lateral, columns of the cord and locomotor ataxy (posterior sclerosis) in the forming stages of the two disorders. In both there are often tinglings, and occasional numbness, and ready fatigue after slight muscular exertion. In locomotor ataxy these are, however, very often accompanied by ocular troubles, as weakness of sight, defective accommodation, strabismus, ptosis, or double vision. In the spinal form of disseminated sclerosis these are wanting, and when they happen in the cerebral, or cerebro-spinal form, they are always persistent, while in locomotor ataxy they are generally temporary. The terrible boring, gnawing, or lancinating, pains of the ataxic, so frequently preceding marked motorial troubles, are rare in disseminated sclerosis. The course and physiognomy of the two diseases in their developed state, when one does not complicate the other, are too distinct to allow of any confusion. The characteristic hitchy, catching-up gait of the ataxic, resulting from impaired muscular adjustment, has nothing in common with the paraplegic shuffling of Charcot's disease. Even in the paretic stage of multilocular sclerosis, the gait is distinctive; the lifted leg is brought forward, not in a direct antero-posterior plane, as in the physiological act of walking, but the foot describes an arc of a circle, and from within outward, at first; and then, having reached the extreme point of the curve, it is thrown inward, coming to the ground with a flap, the sole striking at all points at the same time. With this excentric curvilinear projection of the foot, there is a cadenced oscillation, an exaggerated alternate semi-rotation of both halves of the pelvis (Jaccoud). Early paresis passing, certainly, and more or less speedily, into paralysis, characterizes multilocular sclerosis; while paraplegia is always a very late phenomenon in ataxy, and in nowise a necessary one, so long as the tissue-changes are limited to their own district. When the posterior columns of the cord are invaded by the sclerotic patches, already existing in the anterior, or antero-lateral, then the signs of the two affections will coexist, but the phenomena proper to each can generally be separated by careful examination.

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[23] In the hand, the contortion is very characteristic. At first, the phalanges are partly flexed on the metacarpal bones, and drawn toward the thumb, as if the patient were holding a pen, or about taking a pinch of snuff. After a while this condition is aggravated, the first phalanges of the fingers are closely and tightly bent, while the second are in a state of permanent hyperextension; on the first and the third in a slight degree, of flexion on the second (Ordenstein).

Course and Duration.—The course of this affection is progressively invasive, whether it begins in the lower or upper extremities, or in the head. Muscular enfeeblement slowly but certainly increases, until all the voluntary muscles are more or less disabled. Tremor, at the outset slight, partial, and occasional, comes to be violent, spreads over the whole body, and is started on the least voluntary movement, or by any emotion. Convulsive agitations of the extremities, spells of rigidity, and permanent contraction of the muscles succeed; nutrition troubles set in, and prepare the way for some acute intercurrent disease which is commonly fatal. But there are a few exceptional instances in which the advance was not continuous. In one, there were repeated pauses and recommencements before the disease became regularly progressive (Vulpian). Leo reports a case in which the palsy, at first uni- and then bilateral, disappeared for a while altogether. One patient improved so much under the use of strychnine, that he could stand without support, which he had not been able to do for some time; and this continued for two months (Zenker). Another recovered of the paraplegia, and rectal and vesical paralysis, and during six months could walk alone, when the symptoms started afresh (Bourneville). Dr. Charcot relates the case of a woman, who for two years was obliged to remain almost completely motionless, the limbs being permanently contracted, with scarcely a few short intervals of relief; at the end of that time she became able to walk about and attend to her household duties; when, after a fit of convulsive hysteria, all her limbs and the muscles of the trunk became rigid, and remained persistently so to her death, nine years later (*Gaz. Hebdom.*, February, 1865). In two cases there was decided amendment on the appearance of the catamenia, and which ceased on their stoppage. Another remarkable case of repeated pauses, improvement, and aggravation, is reported by Vulpian. During an attack of small-pox, both paraplegia and contraction ceased in the eruptive stage, and after convalescence all signs of the disorder had vanished. This lasted for three years, when, after a fright, which suppressed the menstrual flow, slight paresis was felt; this passed off, however, on the reëstablishment of the catamenia. Three years subsequently this patient had an attack of jaundice; the motor troubles recommenced, and, after some fluctuations, pursued a regularly onward course.

The average duration may be stated at from eight to ten years. In seventeen fatal cases, it lasted for two years in 2, five to ten years in 9, thirteen years in 2, seventeen years in 1, twenty years in 1, and twenty-four years in 1. The age of death in these cases was: from twenty-five to thirty, 2; from thirty to thirty-five, 5; from thirty to forty, 4; from forty-one to forty-five, 2; from forty-six to fifty, 2; from fifty-one to fifty-five, 1.

Causes and Pathogeny.—It is essentially a disease of adult life. In the eighteen cases collected by Bourneville, fourteen were between twenty-six and thirty-six years of age; and of these, fifteen were females; but it must be borne in mind that, of the physicians who have busied themselves with the study of this affection, nearly all were attached to institutions special to women, the Salpêtrière, for example. In thirteen cases in which the occupations were known, there were: 1 physician, 1 theological student, 1 male teacher, 1 female teacher, 1 pianist, 3 seamstresses, 3 domestic servants, 1 truck gardener (female), 1 flower-woman. Several would seem to have suffered from previous nervous disorders, as hysteria, hemicrania, etc. Excess in alcoholic drinks is mentioned in one case. A fall immediately preceded the onset in another (Vulpian). In three instances it appeared during pregnancy. Once it was developed after violent fatigue. Exposure to damp cold may probably be named as one of the few known causes; it is particularly mentioned by Parkinson as giving rise to paralysis agitans. There is no evidence to show the influence of heredity; the father of one patient is reported as having had tremor of the head at fifty.

It must be allowed that the *pathogeny* of this affection, as well as that of other kindred diseases of the great nervous centres, is obscure and unsettled. May we not possibly find a truer interpretation of their nature by admitting their connection with and dependence on some latent but distinct condition of the constitution of the body, which favors the development of this peculiar form of morbid action? That they are simply varieties of chronic inflammation of the brain and spinal cord cannot well be admitted. They are more than this. The common assigned causes seem insufficient for their genesis without some impalpable preëxisting tendency. May they not be like Bright's and Addison's diseases, local expressions of a general state, in which a specific diathesis enters as an essential factor? When sclerosis of the posterior columns of the cord (locomotor ataxy) comes to be treated of, this theory of the pathogenesis of the class of disorders under consideration will be more amply examined, and the evidence in its favor more largely set forth. Should this view gain favor at any time, its practical application is clear, and it will necessarily be followed by an entire change in the therapeutics of these disorders.

Prognosis.—No instance of recovery has been reported. In the actual state of our knowledge, it may be said to be incurable, and progressively fatal.

Treatment.—Electricity, blisters, counter-irritants, ergot, arsenic, and belladonna, have all been used, without good effect. Chloride of gold and phosphate of zinc aggravated the symptoms (Charcot, Vulpian). Strychnine temporarily modified the tremors in five cases. Nitrate of silver appears to have done the same, but it does harm if there is muscular contraction, or clonic spasms. One case is reported as improved by tonics and sulphur-baths. Dr. Pennock was benefited by hydrotherapy. The constant galvanic current should in all instances be fairly tried. If the writer's suggestion as to the diathetic character of this and similar disorders should prove correct, it may be possible, at least in the early stage, to arrest the progress of this affection, by a method of treatment founded on this theory. This practical point will be more particularly discussed and applied in a subsequent article.

APPENDIX.

CASE I.—*Spinal Form.* (Reported by J. C. MORRIS, M. D. Microscopical Examination by S. WIER MITCHELL, M. D. Transactions of the College of Physicians of Philadelphia, 1868.)

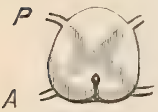
Dr. Caspar W. Pennock, of Philadelphia, of large frame and muscular development, began, in the winter of 1842–'43, when about forty years of age, to feel numbness and a sense of heaviness in his left leg. The sensation was that of a tight band around the leg, three or four inches in width. He had been laboriously occupied during the summer with professional duties, which severely taxed both his mental and physical powers. During six years, in spite of counter-irritation to the spine, galvanism, and hydrotherapy, (which latter seemed to have some temporary effect), the symptoms increased, extending to the whole limb, and then appeared in the right lower extremity; and in 1849 he had become gradually paraplegic. In 1853 he was attacked with apparently phlegmasia dolens in the lower limbs. It ran its course in three weeks, leaving him ever afterward unable to walk, and he was entirely confined to the sitting or horizontal position. In time the paralysis reached the upper extremities, beginning in the left arm, and then spreading to the right, and becoming so great as to prevent him during the last ten years of his life from feeding himself. Sensibility of the paralyzed limbs was somewhat lessened, but never entirely lost. Five years before death there was temporary paralysis of the bladder. His intellect was uninterruptedly clear and bright to the latest period of life, and he took a deep interest in every thing. Death from acute intercurrent disease in 1867, twenty-four years after invasion. Course of disorder regularly progressive. There was total loss of voluntary motor power below the neck. Reflex action intact.

Autopsy.—The cerebrum, cerebellum, and pons were without notable change. At the upper part of the posterior face of the medulla oblongata, in the mouth of the fourth ventricle, lying between the restiform bodies and posterior pyramids, there was a small, irregularly-rounded concretion, of two lines in diameter.

The cervical and dorsal regions of the spinal cord presented in the fresh state, to the naked eye, a series of gray translucent spots, of irregular form, and sometimes almost transparent. They were all a little sunken below the level of the surface of the cord.

The microscopic examination of these spots, which lay chiefly in the white substance, showed—1. Total absence of nerve-tubes and nerve-cells; 2. Finely-granular matter, molecules, and small globules of fat in great quantity; 3. No granulation-corpuscles; 4. Numerous fibres. The vessels of the cord were everywhere altered by fatty deposits, which were particularly conspicuous in the neighborhood of the spots. No vessel was detected in the altered tissue. Excepting in a small spot which invaded the right posterior horn of gray matter at the level of the first cervical nerve, and a like spot on the left side in the same situation, the change respected the posterior horns and columns of the cord. The lateral columns were extensively affected, the spots, as a rule, lying between the anterior nerve-roots, and the central line of the lateral columns, involving, therefore, most largely, the parts nearest the anterior nerve-roots (see Figs. 6 and 7). In three places the changes passed across the anterior columns. From the seventh dorsal to the eighth dorsal nerve, a large spot extended across both the anterior columns; its depth and form are seen in Fig. 5. About the level of the tenth dorsal, and at the second lumbar nerves, two spots were found crossing on to the right and left anterior columns respectively, but not involving the whole width of either. Below these points no spots were found in any part of the cord. The most extensive lesions were in the lateral columns of the cervical cord. Their surface extent is shown in Figs. 6 and 7. Their depth and the parts affected in the interior of the cord are seen in Figs. 8, 9, 10, 11 and 12.

Fig. 5.

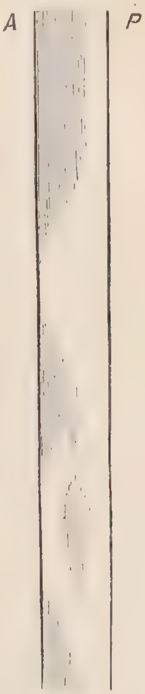


Between seventh and eighth dorsal.

In the dorsal region the spots were abundant, but less numerous than above, about half as many for equal areas of tissue. In many portions both of the cervical and dorsal regions, the changes involved the point of entry of the anterior nerve-roots and the gray matter of the anterior horns (*see* Figs. 10, 11, and 12). The central canal was distinct throughout its course.

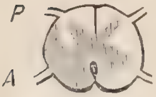
Fig. 6.

Fig. 7.



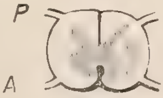
Right and left lateral surface view of the lesions of the cervical cord.

Fig. 8.



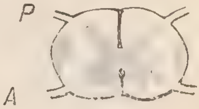
3d and 4th.

Fig. 9.



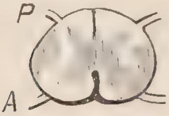
5th.

Fig. 10.



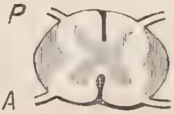
6th.

Fig. 11.



7th.

Fig. 12.



8th.

Cross-sections of cervical cord, showing lesions at the level of the nerve-roots, indicated by the numbers.

CASE II.—*Spinal Form. No sclerotic patches visible to the naked eye on the examination of the cord. The microscope shows the characteristic lesion.*—(CHARCOT; reported by BOURNEVILLE.)

C. C., female, aged forty-one, seamstress, widow. Intellect very feeble, and memory almost gone; not able to give any history of her case. Entered hospital February 18, 1868. Face pale, pupils natural; vision a little weakened; speech embarrassed, and accompanied with tremor of the lips; stiffness of muscles of the neck; hands cold and purple. In a state of repose, nothing abnormal in upper extremities. Rigidity of the shoulder and elbow-joints. Holding her snuff-box in the left hand when she attempts to open it with the right, it is at once affected with tremor. When she attempts to carry a glass of water to her mouth, holding the vessel in the right hand, she always fails after repeated efforts. Muscular power diminished in the right upper limb; sensibility to pinching and pricking appears natural. The thighs are flexed on the pelvis at a right angle; the legs on the thighs; the lower extremities regarded as a whole are in adduction, and that to such a degree that the knees can be separated from each other but very slightly, without causing great pain. It is possible to extend the legs somewhat without causing pain to the patient. If, after extending the right leg as much as possible, it is raised from the bed, the foot begins to tremble; and, if at this time the sole of the foot is tickled, there is produced a sudden spasm in the whole limb, as from an electric shock. The different kinds of sensibility natural in both lower limbs. Although from the mental state of the patient the commemoratives were wanting, the diagnosis, made with some reserve, was spinal disseminated sclerosis, based on her condition when admitted into the hospital; the paraplegia and permanent contraction of the lower extremities, the tremor on voluntary movements, the spinal epilepsy, etc. The absence of trembling of the head and of nystagmus seemed to show that the brain was not implicated.

Autopsy.—No patches of sclerosis were visible in any part of the cord; but on microscopic examination of sections of different portions of the cord, the existence of sclerosis was made certain by the histological changes present. There was considerable connective-tissue proliferation, in certain districts, with fibroid substitution, but the nerve-tubes were yet generally intact. "Are we," asks the reporter, Dr. Bourneville, "from this case to conclude, by those lesions not being appreciable to the naked eye, that there may be several varieties of sclerotic patches? or that the change was yet in its earliest period?" This latter hypothesis seems, from the histological appearances, to be the correct one. Besides, the same thing has been already seen in sclerosis of the posterior cords (locomotor ataxy), as in the instances reported by Dr. Gull, of London, and Drs. Charcot and Bouchard, in which no tissue alteration apparently existed, and the microscope revealed the characteristic changes.

Case III.—*Cerebro-Spinal Form.*—(Charcot; reported by Bourneville.)

H. B., female, aged forty-one, admitted January 6, 1868. Health delicate in youth; after fourteen, when menstruation began, good. Married at twenty-three; has had three children. At the age of twenty-eight, during second pregnancy, had a fall. When about thirty, after a long walk, first felt weakness in the legs; this gradually increased, so that in two years she could not walk without aid; at this time there was incontinence of urine and fæces. Under a tonic treatment and sulphur-baths, the paralysis of the sphincters disappeared, and her lower limbs became so much stronger that she could walk without support, but could be easily thrown down. This improvement lasted for six months, when the paraplegia returned and regularly progressed. The lower limbs felt weighted and stiff. During her third pregnancy walking became impossible; she had to be lifted in and out of bed. About nine years after onset she began to have a sensation of fatigue in both upper extremities, and in the same degree. The rigidity of the lower limbs increased. Her condition on January 7, 1868, day after her admission to hospital: General health and nutrition good; constipation; menstruation unaffected. Rigidity of lower extremities, which are in a state of adduction; rigidity most marked on left side; motility completely abolished; sensibility to pricking, pinching, tickling, and cold and heat, natural; the latter, perhaps, slightly increased; no pains in limbs, no formication or numbness, but a constant feeling of fatigue and heaviness; no palsy of sphincters. Severe pain, almost continuous, but with exacerbations, in the lower lumbar spine and sacrum; small bed-sore over sacrum.

Both upper extremities are weak, the left more than the right. When quite still, no tremor noticeable; but so soon as she attempts to carry any thing to the mouth, there is trembling of the left side, and the head is at the same time agitated.

There had been for some time, exactly how long the patient was unable to say, weakness of sight, greater in the left than the right eye, with occasional diplopy; no periorbitar pains; no ocular delusions.

January 17.—Pains around the base of the chest, hindering respiration, and more marked on the right side. Shooting-pain spells, during which there is slight congestion of the face; the neuralgic points are: 1. To the right of fifth dorsal vertebra; 2. Below the right breast; 3. Slight hyperæsthesia at epigastrium.

January 31.—Redness in the metacarpo-phalangeal and finger-joints, with pain, increased on motion. Sacral sore larger. Rigidity of lower limbs, as well as tendency to adduction, lessened. Severe pain at times in instep, compared to the part being squeezed in a vice.

February 5.—The lower extremities, which on the day previous were without any rigidity, are drawn up, and to effect extension requires much effort, particularly on the left side, and they become immediately again semi-flexed. On pinching the skin of the thighs and legs, and tickling the soles of the feet, the legs become rigid and bent on the thighs, especially the left, and reflex movements are exerted, which throw the limb upwards. Application of cold produces the same phenomena, but to a less degree. The greater the degree of flexion, the less the tendency to adduction. When the sole of the foot is tickled for some time, tetany is induced in the limb, which lasts for some time after the excitation has ceased. Occasional subsultus in the lower limbs. After a while, the rigidity and contraction pass off, and the position of the limbs becomes natural. A constrictive pain around the hips, from the sacrum to the pubis, complained of.

In the upper extremities a feeling of uneasiness is experienced from time to time, when the hands cannot be used, and there is tremor on voluntary movement. About this time, ascending spinal meningitis from the sacral slough set in. On February 16th, the pulse-rate was 100 to 120; respiration, 36 to 40; body heat, 39°5 C. to 40°1 C. February 20th.—Involuntary stools; lower extremities flaccid and handling, and kneading them, even, do not cause contraction; tactile sensibility natural; temperature of lower extremities less than of upper; pupils normal; sight a little dim.

On the 4th of March pneumonia set in, with diarrhœa and disturbed digestion. Up to this time no headache had been felt, but now, when the head is moved, there is a deep-seated sensation in the upper part of the occipital region as if a blow had been received there, and this lasts for an hour at the time. Memory good; no disturbance of ideation; no feeling of heaviness, or of increase or diminution in the size of the head; no stiffness of the neck.

March 23.—Contactile and thermic sensibility perfect and without retardation in the lower limbs, which are relaxed; a little stiffness in the knees; some tendency to adduction; no voluntary movements, even of the toes; reflex movements exaggerated; on tickling the sole of the foot, the leg becomes flexed on the thigh, and the thigh on the pelvis; and if the excitation is continued for any time, a series of convulsive movements is provoked in the limb, which continues bent. During the night involuntary muscular startings in the lower extremities, in the direction of flexion, are frequent.

There are spells of increased weakness of the arms; the trembling of head worsens; new sloughs form; the lumbar pains are more intense; evacuations involuntary; diarrhœa; dilatation of the pupils, the right more than left; objects placed at the right of the eye appear double with both eyes, but are correctly seen by the single eye. The lung-trouble becomes developed, and the patient dies April 6, 1868.

Autopsy.—Patches of sclerosis in the cerebral hemispheres and pons varolii. In the spinal cord they are scattered throughout the whole length, and in every part except the posterior columns.

The association of fasciculated sclerosis of the posterior columns of the cord with disseminated sclerosis of the anterior and antero-lateral columns, is shown in the two following cases, reported by Friedreich in his paper on "Atrophic Degeneration of the Posterior Columns of the Spinal Cord" (locomotor ataxy), published in Virchow's Archiv. fur Pathol. Anat. u. Physiol., u. für Klin. Medicin., 1863, pp. 419–433.

Case IV.—(Friedreich.)

J. S., female: first felt weariness, then weakness of right leg at sixteen years of age; and at the same time severe shooting, intermittent pains in the lower extremities. When about twenty, she noticed increasing weakness of the right arm, and soon after of the left, while the paralysis of the inferior extremities grew worse. Nine or ten years later, severe pains were felt in the ends of the fingers, but more in the right than the left hand; about this time there were convulsive startings in the muscles of the leg, particularly while in bed, which would cause involuntary flexion of the legs on the thighs, and the thighs on the pelvis. Eleven years afterwards the head became tremulous as soon as it was raised from the pillow, and there was bilateral nystagmus, when any object was steadily looked at. Speech was embarrassed to a degree to be almost unintelligible (æt. 21); no headache, but vertigo; spasmodic contractions of lower limbs; ataxia. Death from typhoid fever, fifteen years after onset. Sensibility and vision had remained intact. At the autopsy, sclerosis of all the columns of the cord was found.

Case V.—(Friedreich.)

N. S., female: was seized with feebleness and lancinating pains in both lower extremities at the age of seventeen. Towards twenty the paraplegia increased, and a sense of heaviness in both arms was felt, but no pain. At twenty-six, speech became affected, and there was stuttering. The pain-spells in the lower limbs recurred from time to time, until her entrance into the hospital, when she was twenty-eight years of age. During the two previous years there had been occasional spasms of the peroneal muscles, and within a year severe frontal neuralgia, lasting sometimes for a whole day. On the day she came to the hospital, her speech was almost unintelligible; voluntary motion of arms difficult and uncertain, so that she could not button her clothes, or thread a needle, or squeeze an object only after many attempts; there was small control over the muscles of the arms; she could neither stand nor sit; electro-sensibility and contractility natural; in the lower extremities, strong currents, which induced violent contractions, caused less pain than in the upper; cutaneous sensibility, tested by æsthesiometer, everywhere good. Special senses natural; slight nystagmus; no paralysis of the muscles of the face; right cyphosis and scoliosis of dorsal spine. Death from typhoid fever a few months after admission. Examination of the spinal cord showed extensive sclerosis of the posterior columns, and the posterior nerve-roots were atrophied. In a segment of the cord adjacent to the lumbar enlargement, although the naked eye could detect no lesion of the lateral columns, the microscope showed connective proliferation analogous to that in the posterior column; there was fibrillary tissue, with some atrophy and decided varicosity of the nerve-tubes. In the pia mater of the cervical region and upper part of the medulla oblongata, there was intense yellowish-brown pigmentation, though no alteration of the posterior columns could be made out at these levels. [It does not appear that those portions were examined microscopically; had they been, the essential tissue-changes would, most likely, have been seen.] On exposing the fourth ventricle the ependyma of the inferior half of the rhomboid fossa, near the cord, was found to be much thickened and hard [a patch of sclerosis probably?], while the upper part was normal.

ART. II.—*The Prognosis in Chronic Diseases of the Heart.*[1]

By AUSTIN FLINT, M. D.

IN my remarks on "The Prognosis in Chronic Diseases of the Heart," I shall have reference especially to the organic affections which constitute the great majority of those occurring in medical practice, namely, valvular lesions and enlargement of the heart. Fatty degeneration of the heart, an affection not very infrequently met with, will claim notice. Congenital malformations, chronic pericarditis, and certain anomalous lesions, are comparatively rare, and cannot be separately considered in a paper so limited in length as this must be. To these affections, however, the remarks which will be offered in relation to the prognosis in cases of valvular lesions, enlargement of the heart, and fatty degeneration, are in the main applicable; hence, their separate consideration would not perhaps be important, even were the length of the paper not limited.

The prognosis in cases of diseases of the heart is peculiarly important as regards the communication of medical opinions to patients or their friends. It is a popular belief that all cardiac lesions are alike dangerous, always involving a liability to end fatally at any moment. A patient known to have an organic disease of the heart is looked upon as one under sentence of death, the execution to take place at any time without warning. Now, it is needless to say to this audience that there are lesions which are devoid of immediate danger; that, in respect of gravity, different lesions and different cases of the same lesion differ widely, and that sudden death from disease of the heart is the exception, not the rule. But it must be confessed that the popular belief, just referred to, is not wholly confined to non-medical persons; it pervades, to a certain extent, the medical profession. This is owing to the fact that the development of much of our present knowledge of diseases of the heart is of recent date; and also to the fact that many give but little attention to these diseases, under the erroneous idea that the study of them is too difficult to be undertaken, except by those who design to make it a specialty.

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[1] Read at a meeting of the New York County Medical Society, March 7, 1870.

In diseases of the heart, fully as much as, if not more than, in other classes of disease, the prognosis must be based on the diagnosis. And it is with reference especially to diagnosis that our knowledge of these diseases has, within late years, been greatly advanced. This advancement has consisted in the successful application of physical exploration, to an extent which could hardly have been dreamed of by the illustrious founder of auscultation. At the present moment, thanks to auscultation and percussion, the diseases of the heart are discriminated with more promptness, precision, and positiveness, than those of any other province in the nosology. The wonderful accuracy with which, by means of physical signs, cardiac lesions are now detected and differentiated constitutes a basis for prognosis vastly broader and more solid than formerly existed; yet, fundamental in importance as is the diagnosis, our predictions are by no means to rest on this alone. In the course of the remarks which are to follow it will be seen that too exclusive reliance on the information afforded by physical signs, is apt to lead to errors in prognosis, which are sometimes so prejudicial to the welfare of patients, that it would have been better had the information not been obtained. It is a strange but nevertheless a true statement that the perfection of the physical diagnosis of diseases of the heart may be made to do harm rather than good. This, however, arises from not duly appreciating circumstances which are to be taken into account in conjunction with the diagnosis. Diagnosis, as I have said, is the basis of prognosis; but clinical experience furnishes the materials for building the latter upon the former. Practical skill in prognosis implies, in addition to accuracy of diagnosis, sound clinical experience. The two qualifications cannot be disjoined; the judgment of one whose knowledge is limited to diagnosis, be he never so skilful in this department of our art, is often unreliable; and, on the other hand, the opinions of the clinical observer without claims as a diagnostician have no secure basis.

The topic which first suggests itself, in entering upon my subject, is the importance of determining the existence, or otherwise, of lesions of any kind in cases of a chronic affection of the heart. A patient has symptoms referrible especially or chiefly to this organ. Its action is disordered, as shown by undue violence, irregularity or intermittence, and these symptoms may persist, or recur at short intervals, so that there is an affection which may be said to be chronic. To the symptoms just named may be added dyspnœa, a sense of oppression or undefinable distress referred to the præcordia. Under certain circumstances, general dropsy occurs, the kidneys being free from disease. Now, with these symptomatic phenomena, cardiac lesions do not necessarily exist; the heart may be sound, the disturbance being purely functional. Even the affection known as angina pectoris may be well marked without being associated with organic disease of the heart. It is probable that this affection may occasion sudden death without any coexisting cardiac lesions, although the instances must be exceedingly rare. Excepting these rare instances of angina pectoris, we are warranted in saying that, if the affection of the heart be purely functional, there is no danger; we may commit ourselves unreservedly to a favorable prognosis. The simple question, then, as regards the prognosis, is, Are there, or are there not, cardiac lesions? This question is to be settled by physical exploration. Here diagnosis is in itself sufficient and supreme. Does a careful examination by auscultation and percussion show an absence of all morbid signs denoting lesions? We may declare the affection to be purely functional, and on this basis give positive assurances of the absence of danger.

It is certainly an unfortunate error to pronounce a verdict of organic disease of the heart in cases of purely functional disorder, the more because the disturbance of the heart's action, in such cases, were it attributable to lesions, would denote more or less immediate danger. Nor, on the other hand, is it an error without unpleasant consequences, as regards the reputation of the physician, if not the welfare of the patient, to declare, in cases of organic disease, that there is only a functional malady. Both these errors cannot fail to fall to the lot of those who venture upon a positive diagnosis without the aid of physical signs. It is sometimes hardly less unfortunate, as regards the moral effect upon the patient, if, thinking that "discretion is the better part of valor," the physician refrains from hazarding a definite opinion; for the patient who may have only a functional affection infers that he has organic disease, and that the physician is unwilling to communicate the fact of its existence.

An important aspect under which the prognosis of chronic diseases of the heart is to be considered is the innocuousness of certain lesions. Lesions of the valves, as is well known, are represented by adventitious sounds known as endocardial murmurs. By means of these murmurs the existence of valvular lesions is determined, and they are readily localized. If there be found, in any case, endocardial murmur or murmurs persisting, and not due to a morbid condition of the blood, we have the proof of a chronic structural affection; there is organic disease of the heart. But the lesions which give rise to murmurs are by no means always of importance as regards immediate or even remote evil consequences. They may be devoid, not only of danger, but of any morbid symptoms. There are many persons pursuing their various avocations, and wholly unconscious of any malady, who, if auscultated, would be found to have organic disease of the heart. In a certain proportion of these persons the existence of cardiac disease will hereafter be manifested by symptoms and morbid effects; some may at length die from the disease, but in not a few, even if life continue for many years, the only evidence of the disease will be, as now, the presence of one or more of the cardiac murmurs, and death will be caused by some affection which has no connection with the lesions existing in the heart. In cases of innocuous lesions the harm of physical diagnosis is sometimes apparent. Let the simple statement be made authoritatively to one having an innocuous lesion that he has an organic disease of the heart, and he will be likely to look upon himself as doomed. If he be a timid, nervous man, he has received a moral blow from which he does not recover. He sees a sword suspended over him. He is under sentence of death. Not only is he hurt as regards his comfort and happiness, but the depressing effect of the diagnosis, and the altered habits of life to which it may lead, sometimes contribute to impair health, and tend, perhaps, to shorten life.

I would not for an instant have it supposed that I mean to disparage physical diagnosis. I wish only to place in a strong light the importance of going further than to the fact of the existence of organic disease of the heart. In other words, I would prepare the way for saying that, with reference to the prognosis, more information than the murmurs can furnish is indispensable. What has just been said concerning the long-continued innocuousness of cardiac lesions, I may add, is warranted by my own observations. I have records of cases in which organic endocardial murmurs existed from ten to thirty years ago, the persons now living, and exempt from ailments referrible to disease of the heart.

It is difficult, without the lessons of clinical experience, to appreciate the fact that the intensity and quality of heart-murmurs are not of much account in judging of the importance of valvular lesions. A murmur very loud, notably rough or musical, it would seem, should denote graver lesions than one which is feeble, soft, and blowing. Experience, however, shows that it is not so. A striking illustration of this fact came under my observation some time since. A gentleman from

Cuba consulted me for disease of the heart. He had a musical murmur loud enough to be heard with the ear removed some inches from the chest. The sound had attracted his attention, and this first led him to see a physician. He was told that he had disease of the heart, of which he had previously had no suspicion, having no ailments referrible to that organ, and, indeed, considering himself perfectly well. He became at once a medical curiosity, and he had been examined by many physicians. The case exemplified the fact that the diagnosis of a cardiac lesion is sometimes a misfortune. The man had no peace of mind after the discovery of the murmur. He relinquished his business, and came to this country for medical opinions. The lesion, as regards present importance, was innocuous; and had he remained ignorant of its existence, he would not only have been contented and comfortable, but his condition would probably have been more favorable for the preservation of health.

It follows, from what has been said, that, with reference to prognosis, it is important to go further in diagnosis than to determine, from the presence of murmur, the existence of an organic disease of the heart. If we except the accident of embolism, we are warranted in saying that, as a rule, in cases of valvular lesions giving rise to murmurs, whatever be their number, intensity, and quality, there is no danger, either immediate or near at hand, so long as the heart is not enlarged; for clinical observation shows that, in general, valvular lesions cause enlargement of the heart before leading to more remote effects which involve distress and jeopardize life. Moreover, clinical observation shows that in most cases enlargement of the heart is produced by valvular lesions slowly, the ulterior effects being, of course, proportionately distant. I would remark, in this connection, that, in order to judge of the import of organic murmurs, aside from enlargement of the heart, the heart-sounds claim more attention from stethoscopists than is usually given to them. It is certain that the aortic and the pulmonic second sound can generally be interrogated separately by auscultation; and I believe this statement may also be made with respect to the mitral and the tricuspid valvular element of the first sound. The absence of any abnormal modifications of these several components of the two sounds of the heart is an important point in judging of the innocuousness of valvular lesions, the existence of which is revealed by the presence of murmur.

The compensatory and conservative provisions which are strikingly manifested in the diseases of the heart possess much interest and importance in connection with the subject under consideration. I have just said that valvular lesions, as a rule, are without danger if the heart be not enlarged. Now, hypertrophy of the muscular walls of the heart, taking place as a direct effect of lesions which occasion obstruction to the blood-currents, or regurgitation, or both, is a compensatory or conservative provision. The increased muscular power which the increase of growth gives makes amends for the disturbance of the circulation, and prevents evils which would otherwise ensue. A patient is, comparatively speaking, in most cases safe as regards the serious consequences of disease of the heart, be the heart considerably or even greatly enlarged, provided the enlargement be due to muscular growth or hypertrophy. At all events, this form of enlargement, when associated with, and dependent upon, valvular lesions, is productive of good rather than harm. On the other hand, there is nothing compensatory or conservative in the enlargement due to dilatation; but it is quite the reverse of this. In proportion as the cavities of the heart are dilated, the ability for effective contraction is impaired. While hypertrophy gives increase of systolic power, dilatation adds no muscular strength, but increases the labor in so far as the enlarged cavities allow a larger accumulation of blood. In fact, it is by means of the weakness of the heart, incident to dilatation, that valvular lesions lead to remote evils—namely, those resulting from systemic congestion sufficient to interfere with the functions of the stomach, liver, kidneys, and brain and other organs, and eventuating in general dropsy. Hence, it is evident that, with reference to prognosis, it is highly important to determine whether hypertrophy or dilatation predominate in causing the enlargement which is found to exist. The differential signs, which it would be out of place to consider here, are sufficiently explicit. Let me add that the statement just made concerning the relative importance of hypertrophy and dilatation in prognosis is alike applicable to enlargement of the heart occurring independently of valvular lesions; and there is reason to believe that hypertrophy is measurably a compensatory and conservative provision when it takes place in other pathological connections; for example, when it occurs in the course of Bright's diseases.

It is a curious fact that as muscular hypertrophy of the heart is compensatory and conservative as regards obstructive and regurgitant valvular lesions, so these lesions may be compensatory and conservative as regards muscular hypertrophy. Certain evils, liable to occur in consequence of the increased power of the left ventricle from hypertrophic growth if the valves be sound, are warded off by coexisting valvular lesions. Hypertrophy of the left ventricle, if there be no obstructive or regurgitant lesions, involves a liability to congestive apoplexy, and favors the occurrence of cerebral hæmorrhage; whereas, an overplus of blood sent with an abnormal force to the brain is prevented by aortic or mitral obstruction, or by mitral regurgitation, the immediate effect of which is to lessen the quantity of the blood which otherwise would be driven into the aorta with the ventricular systole. Moreover, statistics show that there is more danger of sudden death from distention with blood and paralysis of the left ventricle, as a consequence of aortic obstructive or regurgitant lesions, when these lesions exist alone, than when they are associated with mitral, obstructive or regurgitant lesions. The latter are compensatory and conservative by preventing an accumulation of blood in the ventricular cavity sufficient to occasion paralysis from distention. A patient, thus, in the first place, with hypertrophy, associated with valvular lesions, is exempt from a liability to evils which hypertrophy existing without valvular lesions may occasion; and, in the second place, the danger of sudden death, which belongs especially to aortic lesions, is lessened by coexisting mitral lesions.

An important topic is the concurrence, with organic disease of the heart, of functional disorder arising from causes which have no connection with the cardiac lesions. I will illustrate the practical point involved in this topic by giving the prominent features of a case:

A young married woman, during lactation, became greatly anæmic. Under these circumstances, she suffered for the first time from palpitation, and she heard at night a sound in the chest, which she described, of her own accord, as a sound like that produced by a pair of bellows, without having had any knowledge of cardiac murmurs. Her sister, who shared her bed, also perceived the bellows-sound. She had œdema of the face and lower limbs, and notable dyspnœa on any exertion. On auscultation, there were found an aortic direct and a mitral regurgitant murmur, both being notably loud. The heart was moderately enlarged. She had had repeated attacks of articular rheumatism. Her friends were apprised of the existence of organic disease of the heart, and the fact was communicated to her. Lactation was at once suspended, and she was placed upon chalybeate tonics, together with a dietetic and regimenal treatment with reference to anæmia. A year afterward this lady presented the aspect of blooming health, and considered herself perfectly well. She laughed at the idea of her having any disease of the heart, and, in conjunction with the attending physician, I was supposed to have fallen into an error of diagnosis. But the murmurs, together with the evidence of enlargement, existed as before, and some six years afterward she died, after having had an attack of apoplexy with hemiplegia.

Here was a case in which there were cardiac lesions giving rise to no appreciable pathological effects or symptoms. Alone, they were not incompatible with excellent health; but, associated with marked anæmia, the lesions seemed to assume great gravity, and the case presented an appearance of an advanced stage of organic disease of the heart. The intensity of the systolic murmurs, the palpitation, the dyspnœa, and the dropsy were attributable, not to the cardiac lesions, but to the coexisting anæmia; and these symptomatic events completely disappeared when the cause of the anæmia was removed, and the normal state of the blood restored by appropriate treatment.

This case is typical of a class of cases in which, superadded to cardiac lesions, are symptoms or pathological events with which the lesions have no causative connection. The symptoms, or pathological events, were they dependent upon the lesions, would denote more or less gravity of disease. But the association is merely one of coincidence. The various causes which produce functional disorder in persons who have sound hearts, are of course operative fully as much, and even more, in persons whose hearts are unsound; and the latter, as well as the former, are liable to be exposed to the causes of functional disorder. Cases in which disordered action of the heart, mainly or entirely functional, occurs in connection with cardiac lesions of little or no immediate importance, are by no means infrequent. The disordered action and the concomitant symptoms are apt to be imputed chiefly or wholly to the lesions in such cases. The prognosis is therefore needlessly grave. Here, again, taking into account the moral effect of the prognosis, it would sometimes doubtless have been better had the stethoscope not been brought into requisition.

It is obviously desirable to determine, as far as practicable, in individual cases, the extent to which functional disorder is independent of existing organic disease. This is not always easy at once. Often, however, there is an evident want of proportion between the lesions and the disturbance of the heart's action. With reference to this point, it is important to form a correct judgment concerning the amount of organic disease. This judgment is to be formed by investigating the cases with reference to the following points of inquiry:

Is the heart enlarged, and, if so, how great is the enlargement? Does hypertrophy or dilatation predominate, if there be much enlargement? What is the information obtained by interrogating the different valves, namely, the aortic and pulmonic, and the two auricular valves separately? Is there ground to infer the existence of fatty degeneration? Again, cases are to be investigated with reference to the existence of well-known causes of functional disorder of the heart, and in this direction these questions will arise: Have pregnancy and lactation preceded the disturbed action of the heart? Has there been loss of blood from hæmorrhages anywhere? Is there anæmia from any cause, or without any apparent causation, as determined, not alone by the complexion, the appearance of the eye, the mucous membranes, etc., but by the venous hum in the neck? Does the patient suffer from dyspeptic ailments? Do mental causes enter into the etiology? Has there been over-excitation of the sexual system? Is tobacco used immoderately? Canvassing fully and fairly the facts embraced in the answers to these two classes of questions, the prognosis is to be based, on the one hand, on the evidence of an inadequateness in the amount of organic disease to account for the symptoms, and, on the other hand, on the adequateness of existing causes to explain the disorder, independently of the lesions which exist.

The difference in the tolerance of chronic affections of the heart is to be considered with reference to the prognosis. What is true of most chronic diseases, namely, that the same lesions are tolerated very differently in different cases, is especially exemplified by the structural affections of the heart. It is truly astonishing how well borne, in some cases, are cardiac lesions of unusual magnitude. A case which recently came under my observation afforded a striking illustration of this fact. The patient, a man of middle age, was suffering greatly from dyspnœa in paroxysms, together with loss of appetite and general prostration, and the case ended fatally within a few weeks after the occurrence of the symptoms just named. I saw the patient a few days before his death, and found the heart enormously enlarged. The apex-beat was in the eighth intercostal space several inches without the linea mammalis; and the dulness on percussion over the præcordia was proportionately increased both in area and degree. Here was truly a *cor bovinum*. There were present murmurs, indicating both aortic and mitral lesions. There had occurred an attack of acute articular rheumatism fifteen years ago. Now, prior to a few weeks before death, this patient had seemed to be in excellent health, and he declared that he was so. He was a man of very active habits, engaged in a business (that of a wool-merchant in the country) which required much travelling. He had had, on one occasion, an attack of hemiplegia, of very brief duration, which was probably attributable to embolism. With this exception, he had not for many years been a patient, considering himself a healthy man. He was a man of temperate habits, but a good liver as regards diet, eating very heartily, and digesting his abundant meals without difficulty; yet, it is certain that for several years there must have been very great enlargement of the heart, resulting from the valvular lesions. For some time before the occurrence of grave symptoms referrible to the heart, he had had an unusual amount of mental and physical work, accompanied with much excitement; nervous asthenia and impaired appetite ensued, and, under these circumstances, he began to suffer from dyspnœa. He was compelled to keep the bed; he became despondent; the existence of disease of the heart was forced upon his attention, and he failed rapidly. The history of this case represents what I have repeatedly been led to observe in other cases, to wit, the tolerance of disease of the heart, while it was advancing, more or less slowly, until it had attained to a great amount, the person affected, in the mean time, not considering himself an invalid, taking no remedies, living freely, and engaged in pursuits involving activity of mind, or of body, or of both. The case also represents a fact which I have repeatedly observed, namely, that from the time when persons with disease of the heart become patients, that is, when they become impressed with a knowledge of the existence of the disease, and are obliged to give up their usual pursuits and habits, they are apt to fail rapidly. It is a *facilis descensus* from that time. The latter fact, as well as the remarkable tolerance of the disease under the circumstances stated, teaches an instructive practical lesson.

In speaking now of the tolerance of cardiac lesions, I do not, of course, have any reference to those which have already been referred to as innocuous. I refer to lesions which are more or less serious, that is, involving either obstruction to the free passage of blood through the orifices of the heart, or regurgitation, or both these immediate effects combined, together with enlargement by hypertrophy or dilatation separately or in combination.

All clinical observers who have seen much of diseases of the heart must have been struck with the fact that the inconvenience and suffering attendant on lesions the same in character and extent, differ widely in different cases.

What are the circumstances on which this variation as regards tolerance depends? This question not only has a bearing on the prognosis, but it is of great importance in relation to management. I will devote to it a few remarks.

In general terms, chronic diseases of the heart, as of other organs, are tolerated in proportion as the functions of the body, exclusive of the part diseased, are healthfully performed. The internal conditions of general health and constitutional strength relate especially to the series of functions which begin with ingestion and end with nutrition. Other things being equal, the toleration is best and longest when, *first* of all, the ingesta are ample; *second*, when digestion is active; *third*, when, owing to adequate assimilation, the constituents of the blood are in normal proportion; *fourth*, when the nutritive supplies in the blood are well appropriated; and, *lastly*, when the secretory and excretory organs do their proper work. Now, a healthful performance of these functions is not incompatible with considerable damage of the central organ of the circulation; and, in so far as it is practicable to maintain these functions at, or near to, the state of health, the toleration of diseases of the heart will approximate to completeness. *Per contra*, the toleration will be incomplete in proportion as the functions of the body, exclusive of the heart, are feebly or imperfectly performed; in other words, in so far as the conditions just named of general health and constitutional strength are deficient. The blood may be considered as representing the healthful performance, or otherwise, of the functions of nutritive and destructive assimilation; so that the simple phrase, *healthy blood*, comprehends the grand requirements for toleration.

In these few remarks on the circumstances on which the variation, as regards tolerance in different cases, depends, I have opened up the governing principle in the management of chronic diseases of the heart. The great object of management in all incurable affections is to prolong and to render as complete as possible the tolerance of them. The prognosis in individual cases will be much affected by a full appreciation of this object, and of the means for its promotion. Here, once more, we are obliged to admit that the knowledge of the existence of cardiac disease is sometimes a calamity. Take the case of which I have given an account, in connection with the topic under consideration. Suppose the patient, whose heart doubtless for a long period was greatly enlarged, had been assured of this fact years before his death; and, with this assurance, it had been enjoined upon him to be abstemious in diet, to watch carefully his digestion, to avoid physical and mental exertion as much as possible, and to await quietly a fatal termination—it is probable that the tolerance, which was such a marked feature of the case up to a short time before death, would have given way long before, that his comfort and usefulness would have been impaired, and his life shortened. It is a rational conclusion that these effects would have resulted from the depressing influence on the mind, insufficiency of alimentation, disordered digestion, and, owing to mental and physical inactivity, defective nutrition, secretion, and excretion. It would be easy to enlarge upon the object of management which has thus incidentally suggested itself, but I must not forget that the subject of this paper is the *prognosis* in cases of chronic disease of the heart. It is evident, however, that, if I do not overestimate the importance of tolerance, as an object of management, and of the means which have been alluded to, for the promotion of this object, the prognosis is in no small degree affected by the practice pursued in individual cases. Here, as in other points of view, treatment is an element in prognosis by no means to be overlooked.

A few words respecting fatty degeneration of the heart. The remarks having reference especially to valvular lesions and enlargement are in the main applicable to this affection. But it is to be remarked that there is a notable difference, as regards diagnosis, between the former and the latter. Not only the existence, but the extent, of valvular lesions and enlargement, may be determined with great precision by means of physical signs. It is not so with respect to fatty degeneration of the heart. This affection has no definite signs. The diagnosis is inferential, being deduced from the evidence of permanent weakness of the heart, taken in connection with the symptoms, age, and other circumstances. It is fair always to give the patient the benefit of doubt or difficulty in diagnosis. If the experience of those whom I address accord with mine, they will be able to recall cases in which fatty degeneration was inferred, and the subsequent history showed the inference to have been incorrect. This is a point to be considered in respect of prognosis, the more because, as will presently be seen, fatty degeneration of the heart belongs among the cardiac lesions which involve a liability to sudden death.

Assuming correctness of the diagnosis, encouragement in the prognosis may be derived from cases in which this lesion existing in a considerable degree is remarkably tolerated. Some years since, a specimen showing rupture of the heart was exhibited at a meeting of a medical society, the rupture arising from fatty degeneration which was great and extensive. The rupture occurred during an attack which resembled *angina pectoris.* Up to this attack the patient had considered himself well, and took active exercise without inconvenience. He had no symptoms leading to the suspicion of any disease of the heart. It is not uncommon in autopsies to find more or less fatty degeneration of the heart when it had not been suspected, death taking place from some intercurrent affection. These facts warrant hopefulness, as regards the prolongation of life, with a certain measure of health, for an indefinite period, even when symptoms and signs denote much fatty degeneration.

In treating of the prognosis in chronic diseases of the heart, some consideration of the liability to sudden death should not be omitted; and my concluding remarks will relate to this topic.

In a very large majority of the cases in which the heart is the seat of organic disease, the cardiac lesions are not, exclusively or directly, the cause of death. Most patients perish from superadded or intercurrent affections which may be either incidental to, and dependent upon, the disease of the heart, or accidentally associated with it. Of the cases in which cardiac lesions are fatal of themselves, that is, in consequence solely of their pathological effects, sudden death occurs in a very small proportion. As already stated, the popular impression is quite the reverse of this; and it is certain that many physicians participate, to a certain extent, in the common belief. The error is sustained by the frequency with which sudden death is attributed to disease of the heart on medical testimony, and after *post-mortem* examinations. It is too much the custom to refer the death to the heart whenever there is evidence of cardiac lesions, either from signs during life, or the appearances in the cadaver. But diseases of the heart, in a certain proportion of cases, do destroy life suddenly. What, then, are the lesions and the circumstances which render patients liable to sudden death?

The affection just noticed, namely, fatty degeneration, may be first mentioned. This affection involves a certain amount of liability to sudden death, rupture being the immediate cause in some cases, but oftener paralysis of the heart from overdistention. Other things being equal, the liability is, of course, proportionate to the degree and extent of the degenerative change; and the amount of the disease can only be determined approximatively by symptoms and signs denoting permanent weakness of the heart's action.

Of valvular lesions, those which occasion free aortic regurgitation involve by far the greater liability to sudden death. The *rationale* is intelligible. The immediate cause of death is paralysis of the left ventricle from overdistention. The interesting fact that mitral regurgitant lesions are conservative, as regards the liability to sudden death from aortic regurgitation, has been stated in another connection. The fact of aortic regurgitation is determined by a diagnostic murmur; but the danger has relation, not to the existence of regurgitation, but to its amount. The latter is estimated by the increased size of the heart, the feebleness or extinction of the aortic second ground, and by the movements of the arteries which, together with certain characters of the pulse, denote that the regurgitation is considerable. It is rare for sudden death to be caused by aortic regurgitation so long as the heart is enlarged by predominant hypertrophy; generally, the weakness due to dilatation is a causative element.

A French writer, Mauriac, has offered an explanation of sudden death in cases of aortic insufficiency, which is, perhaps, worthy of being considered.[1] It is claimed, as preliminary to this explanation, that the blood is forced into the coronary arteries, not by the direct action of the left ventricle during the systole, but by the recoil action of the aorta directly after the ventricular contraction. The state of contraction of the muscular walls during the systole is supposed to constitute a mechanical obstacle sufficient to prevent, at this time, the entrance of the blood into the arteries of the heart. Now, assuming this, if there be much aortic insufficiency, a regurgitant current, caused by the recoil of the arterial coats, takes place, and, owing to the defect of that resistance which is afforded by the semilunar valves in health, the current into the coronary arteries is diminished. In this way, aortic regurgitation involves, in proportion as it is free and abundant, a diminished supply of arterial blood to the walls of the heart; and, of course, the supply becomes more and more diminished in proportion as the systole of the ventricle is weakened by overdistention or other causes, the arterial recoil being weakened in a corresponding degree. Mauriac would ascribe the sudden death to the want of arterial blood in the muscular walls, rather than to paralysis of the ventricle from overdistention. The latter is the *rationale* which I have given, and it is, I believe, correct; but it seems very probable that Mauriac has called attention to a condition which is important as contributing to the occurrence of sudden death. Mauriac's explanation has relation to a topic to be presently noticed, namely, occlusion of the coronary arteries as a cause of sudden death.

Dilatation of the right ventricle resulting from mitral obstructive, or regurgitant lesions, involves some, but a very small, liability to sudden death. The explanation is paralysis from overdistention. Probably the so-called "safety-valve function," at the tricuspid orifice, is a conservative provision against an accumulation of blood in the right ventricle sufficient to destroy life suddenly. It is hardly necessary to say that fatty degeneration of the heart, coexisting with valvular lesions and dilatation, increases the liability to sudden death; but it is not easy to determine this combination during life.

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[1] De la mort subite dans l'insuffisance des valvules sigmoides de l'aorte, Paris, 1860. *Vide* Nouveau Dictionnaire de Médecine et de Chirurgie, 1868, Art. Cœur.

There is danger of sudden death whenever paroxysms of angina pectoris are associated with organic disease of the heart. Other things being equal, the danger is especially great when the angina is associated with aortic lesions which occasion free regurgitation, the mitral valves being sound; and angina is oftener associated with aortic than with mitral lesions. The association with fatty degeneration is also especially dangerous. These facts are easily understood when it is considered that aortic regurgitant lesions and fatty degeneration of the heart involve a liability to sudden death irrespective of angina. What causative agency is exerted by angina in addition to the lesions with which it may be associated? This question can perhaps now be answered satisfactorily, with our knowledge of the effect upon the movements of the heart of galvanism transmitted through the pneumogastric nerves.

Facts appear to show that the force and regularity of the cardiac movements depend on an innervation received through the pneumogastrics. The division of these nerves is followed by notable perturbation of the action of the heart, its movements becoming rapid and feeble. A feeble galvanic current suffices to arrest its action, producing in effect paralysis. Without entering into any discussion of the explanation of these facts, they lead to the rational supposition that, in certain cases of angina, there is superadded to the neuralgic pain a morbid innervation exerted through the pneumogastrics, producing the perturbation of the heart's action which is of frequent occurrence, and sometimes arresting the movements of the heart like the galvanic current.

I have heretofore held the opinion that paroxysms of angina pectoris involved a liability to sudden death, only when there are lesions, more or less serious, of the heart or aorta; and hence, wherever from the absence of physical signs organic disease could be excluded, we are warranted in giving positive assurance of the absence of danger. As a rule, I believe still that this opinion is well founded; but within a short time I have learned by experience that there may be exceptions to the rule, and that the opinion is therefore, in individual cases, to be expressed with a certain amount of reserve. Not long since I saw, with a member of this Society, Dr. Robert F. Weir, a patient who suffered from attacks of angina pectoris. A physical examination revealed no signs of disease of the heart or of the aorta. The patient, however, died suddenly in a paroxysm. On an examination, *post mortem*, there were found some dilatation of the aorta, and some calcareous deposit; but the valves were sufficient, and the heart was neither enlarged nor fatty. It was evident that the lesions had nothing to do with the sudden death, except, perhaps, as entering into the causation of the angina. This case would seem to show that angina may destroy life suddenly, by an inhibitory or paralyzing effect upon the heart, irrespective of cardiac lesions.

The inquiry arises, What are the symptoms during a paroxysm of angina which denote danger of sudden death? In answer, it may be said that there is absence of danger so long as the action of the heart is but little or not at all disturbed, whatever lesions exist, and whether lesions are absent or present. There is little or no danger if the patient have not a sense of impending death, and if the necessity of perfect quietude be not felt. On the other hand, the danger is great in proportion as the action of the heart is rapid, feeble, irregular, or notably retarded.

Is sudden death ever attributable to either embolism or thrombosis of the coronary arteries? It was considered that this might have been the explanation of the sudden death of the late Prof. Enos, of Brooklyn. In that case both coronary arteries were obstructed by calcareous masses, and it was conjectured that, the obstruction taking place suddenly, an arrest of the circulation in these vessels caused paralysis of the heart. It may be assumed that defective nutrition and consequent weakness resulting from obstruction of the coronary arteries, whether due to an embolus, or a thrombus, or the encroachment of calcareous deposit upon the mouths of the vessels, contributes to sudden death, when other causes exist; but it may be doubted whether sudden death is ever attributable exclusively to the occlusion of these vessels. The occlusion of both coronary vessels simultaneously by thrombi or emboli must be an extremely improbable event; but, admitting its occurrence, and a fatal result, the death would probably not be sudden, although it might be speedy. Contractions of the heart may be produced for some time after its removal from the chest, even in cold-blooded animals. If death took place at the heart, in the case of the late Prof. Enos (which is open to doubt, inasmuch as the head was not opened at the autopsy), it seems more rational to suppose the occurrence of an attack of angina pectoris, the arrest of the heart's action being the effect of a morbid agency through the pneumogastric nerves.[1]

Finally, sudden death may be incident to the coagulation of blood in either of the heart-cavities, occurring in cases of weakness from either dilatation or fatty degeneration. The presence of an *ante-mortem* clot, even of considerable size, in either of the cavities, in connection with the lesions just stated, would not be proof that the sudden death was caused by the clot; it would be more likely to be caused by paralysis, from a sudden increase of the accumulation of blood which led to the coagulation. A heart-clot, as shown in certain fatal cases of pneumonia, destroys life speedily, but not suddenly. The accident incidental to heart-clot, which causes sudden death, is embolism of either the aorta or the pulmonary artery. This accident is to be reckoned among the causes of sudden death in chronic diseases of the heart; but it is proper to add that I have not met with a case in which sudden death was attributable to this cause.

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[1] As bearing on the question whether sudden death is ever attributable to occlusion of the coronary arteries, a specimen recently exhibited by Prof. Loomis, at the New York Pathological Society, possesses much interest. The specimen was from a case in which sudden death occurred during convalescence from pneumonia. Both coronary arteries were plugged with cylinders of fibrin, each of which was connected with a fibrinous mass (a thrombus, not attached) as large as a filbert. The heart was sound, and there was nothing else discovered which would account for the sudden death. The left ventricle was not distended, so that paralysis from overdistention was to be excluded. This case, perhaps, demonstrates that the coronary arteries may be simultaneously occluded; and I am free to admit that it renders this explanation of sudden death, in some very rare instances, less improbable than I supposed, when my remarks in relation to this topic were written.

The few remarks submitted in this paper have related, for the most part, to points from which encouragement is to be derived as regards the prognosis in cases of chronic diseases of the heart. I have considered the subject more especially in this aspect because, in the first place, it is desirable that popular impressions, alluded to at the outset, namely, that all cardiac lesions are of necessity fatal, and that all alike involve a liability to sudden death, should be so modified as to be more consistent with our present knowledge of this class of affections; and here, as in other matters relating to medicine, popular impressions having been derived primarily from the medical profession, must receive their modifications from the same source. I trust that I have not done the profession injustice by saying, as I have done already, that the impressions just stated pervade, more or less, the views of physicians as well as the popular mind. In the second place, it is a principle, applicable to all diseases, that patients are entitled to the comfort and advantage of all the encouragement which, in the existing state of our knowledge, can be conscientiously given, on the basis of diagnosis and clinical experience. The duty of communicating the full extent of danger may be sometimes questionable; but there can hardly be a question concerning, not only the propriety, but the obligation, on the part of the physician, not to withhold whatever is favorable in respect of the prognosis.

If, as has been seen in the course of these remarks, the facility with which diseases of the heart are now recognized, by means of physical signs, be sometimes a disadvantage, what an advantage is it, on the other hand, to have derived from the study of these diseases the lessons which it has been the chief object of this paper to set forth respecting the innocuousness of certain lesions, the provisions for compensation, the tolerance of lesions which are more or less serious, and the circumstances which occasion, in a very small proportion of cases, a liability to sudden death!

Art. III.—*The After-treatment in Cases of Peripheral Linear Extraction.* By Prof. A. von Graefe. Translated, by permission of the Author, from a Letter to Prof. Zehender, by R. H. Derby, M. D., Clinical Assistant.

In the after-treatment of cases of peripheral linear extraction, from what I have seen myself and heard from others, most various courses are pursued, and I am convinced that in this lies the ground why many operators, of whose skill there can be no doubt, have not had such favorable results from the peripheral linear extraction as I have attained in the last few years. I therefore take this opportunity to state again the principles of my after-treatment.

First of all, I do not approve of the incorrect bandages that I have seen so often. It is true, if the disposition to heal be good, that it is enough to close the patient's eyelids and tie a handkerchief about the eye, or to make a simple bandage with a padding of charpie. But we ought to base our after-treatment upon the presumption that the disposition to heal is absolutely bad, and establish the details accordingly. Consequently I regard it as especially important—

1. That a fine and soft material be used as compress, and not a coarse charpie with hard threads.

2. That the charpie be carefully arranged, so that, when the hand be laid closely upon the compress, only a flat surface and nothing of the convexity of the eye be felt, and so that a moderate pressure upon the compress shall cause the patient no pain whatever.

3. That the three turns of the bandage over the eye shall not alone keep the compress in its place, but afford a fully equable support for the surface of the eye from the lower to the upper fold of transmission.

When, upon the removal of the bandage, it appears that a light pressure with the tip of the finger upon the upper periphery of the eye occasions pain, it is an indication that the material used for the compress, or the bandage itself, was at fault. If one but reflects upon the condition of things, this is clear. The point is, that the wound be kept in absolute contact, and at the same time the little conjunctival flap be held in apposition to the episcleral surface, that it may unite directly with it. If, instead of the *three*, only *one* turn be applied, and should this one accidentally press (a thing that depends upon the position of the patient's eye when the bandage is applied) through the compress more upon the lower half of the periphery of the eye and upon the centre of the cornea than upon the wound, then the very opposite from what was intended is attained, that is to say, the bandage gives the wound a tendency to gape, and keeps the conjunctival flap away from the episcleral surface. In cases where there is an unfavorable disposition to heal, this may cause anomalies in the process of healing.

Besides this inaccurate application of the bandage, there is a further danger in leaving the same several days unchanged, even when the patients utter no complaint. In this point I have made so many experiments that I can conscientiously recommend my present practice. My rule is to change the bandage on the evening of the day of operation, and again the morning after. After this the bandage may remain twenty-four hours, but still there is no harm in renewing it morning and evening. When the bandage is first changed on the evening after operation, there are almost always upon the edges of the lids traces of blood, tears, secretions from the conjunctival sac, even small particles of cortical substance, which passed beneath the upper lid, and this must be all washed away, even if the patient feels no inconvenience from them, for they may otherwise decompose and become infectious. There is absolutely no objection to lightly opening the lids, and, by candle-light, cautiously looking at the lower part of the cornea. I would caution against an examination of the wound on the first evening, unless one does it for the sake of study.

Further, I wish to protest against a certain apathy in the after-treatment, when the case looks unpromising, that some of the ablest men of the profession show. I regard it of the utmost importance to promptly recognize the first complications, and energetically to advance against them. The patient should never be allowed to have the least sensation of pain in the region of the wound, that is to say, that does not absolutely depend upon the process of healing. Determination of blood (wallung) to the part follows pain, and abnormal influx of the nutritive fluids (säfte zufluss) in the first stage of healing causes injurious processes in the wound. The following considerations seem to me here of importance:

1. The usual pain from the wound, which directly follows the operation, and which is naturally very variable (there may be none at all), should never be so severe that it makes the patients uneasy, keeps them awake, and, instead of being a moderate, easily-tolerated sensation, becomes a rending, burning, piercing, boring paroxysm of pain. Should such a degree of pain be present, a subcutaneous injection of morphine must instantly be made in the temple.[1] If the pain does not now immediately cease, the bandage must be renewed, and, at the same time, the lids bathed for a few minutes with a sponge dipped in cold water; a continued application of cold I regard as undesirable.

2. After the third hour the pain of the wound should be less, otherwise the rules referred to above should be followed.

3. After the sixth hour there must be no *lasting* sensation in the operated eye. There is nothing to fear from a slight prickling pain, lasting for a second, that comes now and then of itself, or when the patient tries to move the eye beneath the bandage. If this be not the case, but a lasting sensation persists, the bandage should be changed, and, if the sensation now returns, an injection of morphine should be made. It should be especially impressed upon the minds of that apathetic class of patients, so largely represented in the hospital wards, who fancy that so important an act as a cataract extraction must be attended with severe pain, that every *lasting* sensation in the operated eye, be it severe or slight, is an abnormal symptom, and to be instantly reported to the nurse. Should these means be insufficient to still the pain from the

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[1] The objection that has been made to these injections, that they cause vomiting, seems to me unimportant. As far as my experience goes, one patient out of twelve only vomits after an injection, and, moreover, with the present method of extraction, vomiting is attended with no danger. Still, if one fears such an accident, an injection can be tried a couple of days before the operation. By this means, inasmuch as this incidental action of the injection depends almost always upon individual intolerance, we can see in which cases the morphine should be given internally, although its action then upon the pain of the wound is far less sure.

wound, then another injection should be made, or, if the patient is full-blooded, and the circulation accelerated, even a small venesection of four to five ounces, a course that is usually first followed in the period of reaction.

The evening after the operation (I operate usually in the afternoon) the patient should feel quiet and disposed to sleep. On no account should he pass a sleepless or uneasy night. If a subcutaneous injection of morphine has not already been made on account of the pain from the wound, then, in order to encourage sleep, a dose of three grammes of chloral hydrat. in mucilage and sugar may with advantage be given.

℞ Hydrat. chlorali, 3,0.
Mucilag. gummi arab.
Syrup. flor. aurant., āā 15,0.
Aq. distillat., 30,0.
M. To be taken at once in half a wineglass of sugar and water.

Our hospital experience has shown that some patients require four grammes, and drinkers five grammes usually, in order to produce a lasting sleep; and, consequently, if the sleep is irregular, it is desirable, four hours afterward, to give an additional gramme, and potatores even two grammes.

If the patient has already had a morphine injection, on account of abnormal pain from the wound (I inject usually one-sixth to one-fifth of a grain of morphine), and still, although the pain has ceased, he does not sleep, two grammes of chloral should be given—to potatores three to four grammes. The advantage of a good night I regard as of the utmost importance, and every care should be taken to secure this. It is for this reason that I give my patients the day before the operation a dose of castor-oil; for, as is well known, after cathartics the action of narcotics is much more regular and prompt.

Ordinarily the period of reaction of the wound occurs between the twelfth and twenty-fourth hours; in the majority of cases, between the fourteenth and eighteenth. If the case be a normal one, this period should not be attended by the least pain, and consequently the appearance of any abnormal sensation at the end of the first night, or toward morning, is of the utmost import, for at this time the most serious complications (purulent processes in the wound) appear. Even if the time for the second renewal of the bandage has not come, it must now be immediately changed, and the condition of the eye be examined. If there be no increased secretion of tears or swelling of the lids or chemosis, and if the cornea has its usual appearance, it is not necessary to examine the wound. It is usually sufficient to renew the bandage, bathe the lids, and make another small injection of morphine. Should the pain continue, and the patient be strong, a venesection of four ounces should be immediately made. If the lower layers of the charpie are moist; if, upon the linen compress, there be opaque masses of secretion (a layer of dried secretion adherent to the edge of the lids is of no importance); if the upper lid be bloated, an energetic treatment must be followed; for now, even if the wound looks perfectly well, suppuration of the wound is imminent in the fullest sense of the word. After carefully washing the lids, I recommend now a cauterization of their cutaneous surface throughout with lapis mitigatus, neutralizing the same, and carefully drying the skin—then immediately a firm-pressure bandage (a monoculus with four turns, the second and third drawn very tightly); if the patient be robust, a venesection of six ounces, and, half an hour later, an injection of morphine upon the temple. Besides this, I give my patients, soon after the venesection, a cathartic of calomel, rheum, elæs. fœniculi āā gr. x.[1] If the patients are not strong, the venesection is omitted, and the dose of calomel is diminished; otherwise the treatment is the same. By this course of treatment, if it is begun soon enough, one sees ordinarily, when the bandage is again changed, and this should be done under such circumstances at least within six hours, distinctly a disappearance of the symptoms, and a return to a normal process.

It is a well-known fact that there are certain patients whose sensibility is so slight that suppuration of the wound may occur unaccompanied by the least pain. This fact is enough to lead us to remove the bandage during the period of reaction, and look after the eye, even if there be no pain, for otherwise it may happen that one finds too late well-established complications that are no longer to be combated. If these threatening local symptoms manifest themselves in such torpid patients, I would especially recommend an energetic cauterization of the cutaneous surface of the lids, which should be repeated when the next bandage is made, a firm-pressure bandage and a cathartic powder. Where there is no pain, I omit the subcutaneous injection of morphine, but make the venesection if the patient is especially robust, and the pulse full.

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[1] If there be no dejection in ten hours, a tablespoonful of castor-oil.

Much has been said on either side concerning the antiphlogistic treatment under these circumstances. I fancy there would have been less discussion if the period had only been definitely settled. The venesection is of service, in my opinion, only during a short initial period; as soon as a suppurative process in the wound is established, I believe it to be useless.[1]

I spoke above of the advantage, in the period of reaction, of cauterization of the cutaneous surface of the lids, and the application of the closely-fitting pressure-bandage, which must be renewed at least every six hours. If the patient is much reduced or marastic, I limit myself to these two procedures, and give internally usually quinine after a simple cathartic.

As to the further treatment I have but little to add. Should the case continue unfavorable (the condition is usually a partial suppuration of the wound with the consequent secondary processes), I then give, especially if there be gastric irritation, on the second day, an emetic, and continue the pressure-bandage and cauterization of the lids. Aromatic fomentations may be applied for a quarter to half an hour before the bandage is renewed, unless they produce a greater tendency to swelling, and then they must be omitted. Formerly I used these fomentations soon after the extraction more than I do now; they are of great use, however, in some of the later complications, as secondary iritis, etc., which I treat according to the generally-accepted principles. The diet I vary according to the strength and habits of the patient.[2]

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[1] Leeches in the neighborhood of the eye, on the temple, for instance, where there is threatening suppuration of the wound, I consider absolutely injurious; back of the ear they are less dangerous, although to the majority of patients their application is annoying. We scarcely ever take blood locally now before the third day.

[2] This is the best guide, and should be carefully followed. There is ab-

In general in the after-treatment are many individual considerations and circumstances to be weighed that I need not allude to. My principal object has been to emphasize the principles which seem to me in general to secure the success of the operation, the neglect of which, perhaps, explains many a reverse. The attention is concentrated at most upon a *few days.* If the whole happiness of the patient's life depends on this, there is reason enough to concentrate all our energies upon the treatment. How much shorter is the period of anxiety and care than by the old method! At the end of twenty-four hours, if there be no signs of suppuration of the wound, then, with careful treatment, this need no longer be feared. When three to four days have passed without accident, the eye need only be protected from external injurious influences and atropine be instilled.[1]

I apply the bandage if the conjunctiva bears it, and the patient is not constantly under observation as a precautionary measure until the end of the first week. The bandage should never be left off suddenly, but at first for an hour at a time. Even now, during the winter, the majority of our patients, including those operated on both eyes, are discharged before the end of the second week.

Clinical Records from Private Practice.

I.—*Cases illustrating the Effects of Bromide of Potassium in the Treatment of Young Children.* Translated from the *Bulletin Générale de Thérapeutique* of November 15, 1869, by J. CUMMISKEY, M. D.[2]

THE sedative properties of bromide of potassium are now generally recognized, and its therapeutic application, becom-

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solutely no use in giving patients, as tonics, food and drink to which they are not at all accustomed, and which are perhaps distasteful to them. Especially in the use of wine, in my opinion, are usually too general principles followed.

[1] I rarely instil atropine before the third day, unless particles of cortical substance have been left behind.

[2] Read before the Imperial Academy of Medicine of France, by E. Moutard-Martin, Physician to the Beaujon Hospital.

ing daily more widely extended, makes it to-day, without doubt, one of the most useful articles of the materia medica. The freedom from danger attending its administration, even in moderately large doses, makes it particularly applicable to the treatment of young children, whose susceptibility to the action of opium and its preparations renders the employment of these agents very dangerous to life.

During the first few months after birth, infants are frequently subject to attacks of extreme sleeplessness and restlessness—some sleeping in the day, and never at night; others hardly at all either in the night or day—exhausting, by their continued wakefulness, all those who are so unfortunate as to be compelled to watch them.

Children in this condition are benefited most promptly and effectually by the use of bromide of potassium, as the following cases will testify:

Observation I.—"Was called in March, 1866, to see a child four months old, who, for the preceding month, could not sleep at night, and who, during the day, had but occasional and very short naps. The child is well formed, and presents all the appearances of health. Its parents are young, and enjoy good health. Notwithstanding the persistent absence of sleep, it has not lost any flesh, its color is good, and its appetite unaffected. The child will not remain in bed, but obliges the mother to carry it about continually. During the day it falls asleep about four or five times—particularly when taken out into the air—but it is sure to awaken at the end of a half or three-quarters of an hour."

Bromide of potassium was ordered twice a day, in doses of three-quarters of a grain dissolved in a little sugared water, and the breast was directed to be given immediately afterward. The first night after the administration of the bromide, the child slept three consecutive hours, and the continuation of the remedy three days longer restored the patient completely to quiet and health. At the end of two weeks the insomnia returning, the mother commenced, without consulting me, to administer the bromide as previously directed, and, as before, natural sleep was again soon restored.

Obs. II.—"A female infant, two and a half months old, well formed, and under the care of an attentive nurse.

"About three weeks ago, the child's sleep began to be disturbed. It would frequently awake crying, and would continue crying for hours at a time, and, when sleep would at last hush its cries, the respite would be of but short duration, for in an hour it would again awake, and and the crying continue as before. In this way the whole night would pass in alternate spells of crying and dozing." This was its condition when I was consulted. I commenced treatment by ordering three-quarters of a grain of the bromide to be taken that evening, and the breast to be given immediately afterward. That night the sleep was calmer and of longer duration, and the cries less painful and less frequent. The medicine was continued for three days in the same dose, but, finding no further progress, I ordered double the quantity to be taken. From this time perfect quiet was restored. The sleep was normal, the awakenings were unaccompanie with cries, and the nurse observed that the appetite had increased. The bromide was continued for four days in grain and a half doses, and then was decreased to the original dose, which was given for four days longer, and then discontinued.

Obs. III.—" A female infant, eight months old, who had always been in good health until it was six months old, when it commenced to be languid, and to grow alternately pale and red; at night it was very restless and fretful. Salivation, and swelling and redness of the gum (at the seat of the incisors of the lower jaw), commenced to trouble it. Between the sixth and seventh month it cut two incisors, toward the seventh month a cough was noticed, and, notwithstanding the cutting of two more incisors, the cough did not abate, but, on the contrary, became very troublesome and spasmodic in character. The parents, who had attributed the cough to the 'teeth-cutting,' now became alarmed, and applied to me for relief."

The spasmodic character of the cough, the absence of *râles* in the chest, and freedom from fever, decided me to administer the bromide of potassium in doses of one grain, three times a day. The cough promptly lost its spasmodic character, and at the end of three days was entirely gone. The nights were calm, and the appetite increased. The medicine was gradually decreased, and six days after the cessation of the cough it was discontinued altogether. For three weeks the child was very well, cutting, in the mean while, another tooth. But about this time the cough returned, presenting the same characters as before, though not so violent.

The bromide was again given, and the same success attended its second administration as did its first.

The cough returned three times, and each time it was rapidly cured by the bromide.

The cough which accompanies dentition is certainly not produced by an inflammatory condition of the mucous membrane of the bronchiæ, but is a spasmodic nervous phenomenon analogous to the vomiting which is seen in similar cases. It seemed to me rational, therefore, to administer a remedy which has a manifest action on the nervous system. It is certain that the cough, in this case, which in similar cases has defied the most popular remedies, yielded completely in three days, and each time that it returned the efficacy of the remedy was the same. Wherever there is diarrhœa, however, my experience satisfies me that it is imprudent to administer the bromide, as this disease is likely to be increased by it. But how does bromide of potassium act in the affections accompanying dentition? Does it act, by virtue of its sedative property, upon the general nervous system, or has it a special anæsthetic action upon the buccal cavity? It is known that the bromide of potassium paralyzes the sensibility of the velum palati, so that, while taking it, the uvula may be titillated with impunity. But its action is extended to every portion of the mucous membrane of the mouth, and thus, during dentition, it relieves by its anæsthetic action upon the inflamed gums as well as by its general sedative action upon the nervous system.

From cases that have come under my observation, I believe myself authorized to state that, in the diseases incidental to dentition (when uncomplicated by diarrhœa or decided inflammatory symptoms), the bromide of potassium will be found very useful, and will often succeed in preventing the convulsions so frequent at this epoch of life.

Obs. IV.—A. D., a male child, was observed by its nurse, a few days after birth, to have its penis in constant erection. By means of warm baths and cataplasms this condition was relieved, and for some weeks the child continued very well.

When about two months and a half old, I was informed that his sleep was very much disturbed, that from time to time he appeared to have spasmodic contractions of the face (whose color changed easily, passing, in a few moments, from paleness to extreme redness), and that the penis was in a state of almost constant erection. Being desirous of satisfying myself of the latter fact, I made it a point, at each of my subsequent visits, to have the child uncovered, and found it to be as stated. Warm baths and camphorated cataplasms were administered, which gave, after a few days, but temporary relief, for the erections returned, and with them the other congestive and spasmodic symptoms referred to. I commenced then to give the bromide in three-quarters of a grain doses twice a day. During the first four days the effect was not very appreciable. I increased the medicine, therefore, by giving an additional three-quarters of a grain in the day (taking care, as in the preceding instances, to order the breast to be given after each dose). On the fifth day the erethism appeared lessened, and on the ninth day the erections had entirely disappeared. The sleep was calm and the alternate pallor and flushing of the skin were no longer observed. The bromide was gradually decreased, and in the course of ten or twelve days was discontinued. The child remained perfectly well for about fifteen months, during which time he passed through the troublesome period of dentition without any thing noticeable occurring. When eighteen months old he became extremely turbulent, having agitated dreams, and awaking from them crying and jumping; his appetite was also very capricious. When twenty-one months old, I was called to see him again, and found him in this nervous condition, and suffering also from vomiting. He was pale, thin, with rings around the eyes and a restless look; the vomiting occurred frequently at night, and the ejecta were at times alimentary matter, at others simply mucus. I learned also that, for a month, the child had had almost constant erections at night, and that during the day the penis was hardly ever completely relaxed. I prescribed the treatment which had succeeded so admirably before, namely, the bromide in three-quarters of a grain doses twice a day. The first effect produced was the complete cessation of the vomiting, and at the end of two days the erections had diminished, and the sleep was calmer. I increased, now, the medicine to one grain thrice a day, and at the end of eight days the sleep was perfectly calm, the appetite was regular, and the vomiting and erections had entirely ceased. The bromide was continued for fifteen days, gradually diminishing the dose three-quarters of a grain every five days. In a month the child was quite another being: he had grown fat, his face wore a good-natured look, his appetite was regular, his sleep was calm, and he was no longer troubled with that sickly and febrile activity which had been the subject of so much concern and trouble to his parents. To-day this child is six years old, enjoys good health, and possesses remarkable intelligence, and though he occasionally exhibits symptoms of nervous erethism, yet there has arisen no necessity for the administration again of the bromide.

CONCLUSIONS.

1. Bromide of potassium, administered in moderate doses, is tolerated, perfectly well, by children of tender age.

2. By its sedative action, it relieves the insomnia of young children, whether this insomnia be calm or accompanied with agitation and cries.

3. Administered to infants suffering from affections during the period of dentition—characterized by agitation, insomnia, cough, etc.—it succeeds frequently in relieving these complications, and by its judicious use may sometimes prevent convulsions.

4. Bromide of potassium should not be given to children suffering from diarrhœa.

5. In certain exceptional cases in which nervous erethism is predominant, bromide of potassium will have a most prompt and decisive action.

Proceedings of Societies.

MEDICAL SOCIETY OF THE COUNTY OF NEW YORK.

Stated Meeting, March 7, 1870.

DR. GEORGE T. ELLIOT, President, in the Chair.

THE President announced the admission to membership of Drs. Morton Folsom, David Coles Carr, and John Dwyer; also the death of Dr. Charles McCaffry, a member of the Society since 1831.

The Report of the Committee on Intelligence was read by Dr. Thompson, and that of the Committee on Diseases by Dr. Raborg.

PROGNOSIS IN CHRONIC DISEASES OF THE HEART.

DR. AUSTIN FLINT, SEN., read an extended paper upon this subject, which appears in another portion of this number of the JOURNAL.

DR. ALFRED L. LOOMIS, after presenting the specimen of thrombi of the coronary arteries referred to by Dr. Flint, made the following remarks:

MR. PRESIDENT: The subject which has been brought before us this evening necessarily elicits a great diversity of opinion; for as yet there are no fixed clinical and pathological data upon which to rest; each observer and writer on this subject holds opinions of his own, based on his personal observation.

It seems to me the questions to be asked and answered are—1. Does heart-disease ever directly cause death? 2. If so, is it a frequent cause of death? And, 3. Is it ever a cause of sudden death? As an answer to some of these inquiries, permit me to present the results of ninety autopsies (in all of which heart-lesions were found) compiled from the *post-mortem* register of Bellevue Hospital by my junior assistant, Dr. B. D. Taylor.

A Synopsis of the Record of Autopsies made at Bellevue Hospital since June 1, 1868, *in which Valvular Lesions, Hypertrophy, and Dilatation of the Heart, Lesions of the Coronary Arteries, and Cardiac Thrombi, were found —setting forth the Suddenness of Death or otherwise, and its Causes.*

I. HYPERTROPHY AND DILATATION WITH VALVULAR LESIONS.

Case I.—Hypertrophy and slight dilatation. Mitral valves atheromatous. Death gradual from Bright's disease.

Case II.—Hypertrophy and dilatation of right heart. Dilatation of left cavities. Atheroma of aortic and mitral valves. Death gradual from Bright's disease.

Case III.—Hypertrophy and dilatation, with aortic insufficiency. Death gradual from Bright's disease.

Case IV.—Hypertrophy (one pound eleven ounces). All cavities dilated. Mitral valve thickened. Tricuspid thickened and insufficient. Death gradual from pericarditis.

Case V.—Hypertrophy and dilatation. Vegetations on aortic valves. Mitral valve thickened and orifice stenosed. Death gradual from uræmia.

Case VI.—Hypertrophy and dilatation. Aortic valves thickened and insufficient. Mitral thickened. Death gradual from phthisis.

Case VII.—Hypertrophy and dilatation. Aortic valves calcified. Tricuspid atheromatous and orifice stenosed. Death gradual from pneumonia.

Case VIII.—Hypertrophy and dilatation of left ventricle. Vegetations on aortic valves. Thoracic aorta atheromatous. Death gradual from heart-lesion.

Case IX.—Hypertrophy and dilatation of right heart. Mitral stenosis (three-eighths of an inch). Death gradual from heart-lesion.

Case X.—Hypertrophy and dilatation of right heart and left auricle. Mitral stenosis (half inch). Death gradual from heart-lesion.

Case XI.—Hypertrophy (one pound six ounces). Dilatation of all cavities. Aortic stenosis and insufficiency. Mitral valve thickened and insufficient. Death gradual from heart-lesion.

Case XII.—Hypertrophy and dilatation. Mitral valve thickened and orifice stenosed (half inch). Death gradual from heart-lesion.

Case XIII.—Hypertrophy and dilatation. Mitral and tricuspid valves atheromatous and insufficient. Death gradual from heart-lesion.

Case XIV.—Hypertrophy (one pound fifteen ounces). Dilatation of both ventricles. Mitral and tricuspid orifices stenosed. Death sudden from heart-lesion.

Summary.—Fourteen cases: Seven deaths from heart-lesion, one of them sudden. No sudden deaths from other lesions.

II. HYPERTROPHY WITH VALVULAR OR OTHER LESIONS.

Case I.—Hypertrophy. Aortic and mitral valves thickened. Death gradual from Bright's disease.

Case II.—Hypertrophy. Aortic valves thinned. Aorta atheromatous. Death gradual from Bright's disease.

Case III.—Hypertrophy of left ventricle. Aortic valves fenestrated. Death gradual from acute meningitis.

Case IV.—Hypertrophy. Aortic valves and aorta calcified. Death sudden from uræmic convulsion.

Case V.—Hypertrophy. Aortic valves thick and rigid. Mitral stenosis from vegetations. Death gradual from heart-lesion.

Case VI.—Hypertrophy. Aortic and mitral valves atheromatous. Death gradual from heart-lesion.

Case VII. — Hypertrophy. Aortic and mitral valves atheromatous. Aorta and pulmonary artery dilated. Death gradual from heart-lesion.

Case VIII.—Hypertrophy. Aortic and mitral valves and arch of aorta calcified. Death gradual from heart-lesion.

Case IX.—Aortic and mitral valves atheromatous, and latter orifice stenosed. Death gradual from heart-lesion.

Case X.—Hypertrophy. Aortic valves calcified and stenosis of the orifice. Death gradual from heart-lesion.

CASE XI.—Hypertrophy. Aortic valves calcified, and stenosis of orifice. Death sudden by syncope.

CASE XII.—Hypertrophy. Mitral valve thickened and rough; free regurgitation. Death sudden from heart-lesion.

CASE XIII.—Hypertrophy. Mitral and tricuspid valves atheromatous. Death sudden from heart-lesion.

CASE XIV.—Hypertrophy. Aortic and mitral valves and whole aorta atheromatous. Death sudden from heart-lesion.

CASE XV.—Hypertrophy. Mitral and pulmonic valves rigid. Death sudden from heart-lesion.

SUMMARY.—Fifteen cases: Eleven deaths from heart-lesion, five of them sudden. One sudden death from uræmic convulsion.

III. DILATATION WITH VALVULAR OR OTHER LESIONS.

CASE I.—Dilatation of right ventricle. Mitral valve insufficient. Death gradual from Bright's disease.

CASE II.—Dilatation of all cavities. Aortic valves thickened and insufficient. Mitral same, and nearly all the chordæ tendineæ ruptured. Death sudden from uræmic convulsion.

CASE III.—Dilatation of right heart. Mitral valve thickened and orifice stenosed (half inch). Tricuspid valve thickened and rough. Death gradual from heart-lesion.

CASE IV.—Same lesions and termination as in No. III.

CASE V.—Dilatation of both auricles (especially left). Aortic and mitral valves thickened, and stenosis of latter orifice. Death sudden from heart-lesion.

CASE VI.—Dilatation. Aortic and mitral valves thickened and covered with vegetations; former insufficient. Tricuspid covered with vegetations. Death sudden from heart-lesion.

SUMMARY.—Six cases: Four deaths from heart-lesion, two of them sudden. One sudden death from uræmic convulsion.

IV. VALVULAR LESIONS.

(*a*.) *Aortic, Mitral, and Tricuspid.*

CASE I.—Aortic, mitral, and tricuspid valves thickened. Death gradual from Bright's disease.

Case II.—Aortic and mitral valves calcified. Tricuspid malformed and fenestrated. Death gradual from gangrene of lung.

(*b.*) *Aortic and Pulmonic.*

Case III.—Aortic and pulmonic valves fenestrated. Death gradual from phthisis.

Case IV.—Aortic and pulmonic valves fenestrated. Death gradual from phthisis.

Case V.—Aortic and pulmonic valves fenestrated. Death gradual from gangrene of lung.

(*c.*) *Aortic and Mitral.*

Case VI.—Aortic and mitral valves calcified. Death gradual from Bright's disease.

Case VII.—Aortic and mitral valves calcified. Death gradual from phthisis.

Case VIII.—Aortic and mitral valves calcified. Death gradual from phthisis.

Case IX.—Aortic and mitral valves calcified. Death gradual from pneumonia.

Case X.—Aortic and mitral valves calcified. Death gradual from œdema glottidis.

Case XI.—Aortic valves covered with vegetations. Mitral valve insufficient. Death gradual from Bright's disease.

Case XII.—Aortic valves thick and fenestrated. Mitral valve covered with vegetations. Death gradual from subacute meningitis.

Case XIII.—Aortic and mitral valves calcified. Death gradual from delirium tremens.

Case XIV.—Aortic and mitral valves thickened and covered with vegetations. Death gradual from apoplexy.

Case XV.—Aortic and mitral valves calcified. Death sudden from croupous laryngitis.

Case XVI.—Aortic valves covered with vegetations. Mitral valve insufficient. Death sudden from uræmic convulsion.

Case XVII.—Aortic valves and aorta calcified. Mitral insufficient. Death sudden from apoplexy.

Case XVIII.—Aortic and mitral valves calcified, and fenestration of the former. Whole aorta calcified. Death gradual from heart-lesion.

Case XIX.—Aortic valves calcified. Mitral orifice stenosed. Death gradual from heart-lesion.

(*d.*) *Aortic.*

Case XX.—Aortic valves thickened. Death gradual from cholæmia.

Case XXI.—Aortic valves and aorta atheromatous. Death gradual from cerebral softening.

Case XXII.—Aortic valves calcified. Death gradual from phthisis.

Case XXIII.—Aortic valves calcified. Death gradual from phthisis.

Case XXIV.—Aortic valves calcified and fenestrated. Death gradual from phthisis.

Case XXV.—Aortic valves calcified. Death gradual from Bright's disease.

Case XXVI.—Aortic valves calcified. Death gradual from Bright's disease.

Case XXVII.—Aortic valves calcified. Death gradual from Bright's disease.

Case XXVIII.—Aortic valves calcified. Death gradual from pneumonia.

Case XXIX.—Aortic valves and aorta calcified. Death gradual from pneumonia.

Case XXX.—Aortic valves calcified. Death gradual from gangrene of lung.

Case XXXI.—Aortic valves calcified. Death gradual from pyæmia.

Case XXXII.—Aortic valves calcified. Death gradual from cerebro-spinal meningitis.

Case XXXIII.—Aortic valves covered with vegetations. Death sudden from firm and short old pericardiac adhesions.

Case XXXIV.—Aortic valves perfectly rigid. Death sudden from cerebral apoplexy.

(*e.*) *Mitral.*

Case XXXV.—Mitral valve calcified. Death gradual from puerperal peritonitis.

Case XXXVI.—Mitral valve calcified. Death gradual from pneumonia.

Case XXXVII.—Mitral valve calcified. Death gradual from pneumonia.

Case XXXVIII.—Mitral valve calcified. Death gradual from pneumonia.

Case XXXIX.—Mitral valve calcified. Death gradual from phthisis.

Case XL.—Mitral valve calcified. Death gradual from phthisis.

Case XLI.—Mitral valve calcified. Death gradual from phthisis.

Case XLII.—Mitral valve calcified. Death gradual from phthisis.

Case XLIII.—Mitral valve calcified. Death gradual from septicæmia.

Case XLIV.—Mitral valve calcified. Death gradual from pyæmia.

Case XLV.—Mitral valve calcified and stenosed. Death sudden from apoplexy.

Case XLVI.—Mitral valve calcified. Death sudden from apoplexy.

Summary.—Forty-six cases: two deaths from heart-lesion, neither of them sudden. Seven deaths sudden; four from cerebral apoplexy; one from uræmic convulsion; one from croupous laryngitis; and one from old pericardiac adhesions.

V. LESIONS OF CORONARY ARTERIES.

Case I.—Coronary arteries ossified. Aortic and mitral valves calcified. Died comatose from inflammation of the lining membrane of the ventricles of the brain and nervous tissue adjacent.

Case II.—Coronary arteries calcified at their commencement. Aortic and mitral valves calcified. Death gradual from gangrene of lung.

Case III.—Coronary arteries completely plugged by thrombi. Death sudden from heart-lesion.

VI. THROMBI OF THE HEART.

Case I.—Thrombus size of a cubic inch in the left ventricle, which is hypertrophied. Death gradual from cerebral softening.

Case II.—Several thrombi the size of hickory-nuts in the left ventricle, which is hypertrophied, and the aortic valves thickened and fenestrated. Death gradual from acute nephritis.

Case III.—A thrombus the size of a pigeon's-egg in the left ventricle. Vegetations on the mitral valve. Death gradual from cerebral softening.

Case IV.—Large thrombus in right auricle. Death gradual from invagination of the ileum.

Case V.—Thrombus of right auricle. Mitral valve calcified. Death gradual from pneumonia.

Case VI.—Large thrombus of the right ventricle. Death sudden from the heart-lesion.

Dr. Loomis resumed:

From this report it will be seen that valvular disease, cardiac hypertrophy, and dilatation, were present in fourteen cases. Of this number, heart-lesions were the cause of death in seven; death was sudden in one, and was caused by stenosis of the mitral and tricuspid orifices.

In fifteen cases valvular lesions with cardiac hypertrophy were present, in eleven of which the heart-lesions were the cause of death; in five of these death was sudden, and the valvular lesions were aortic in one, mitral in another, aortic and mitral in another, mitral and tricuspid in another, and mitral and pulmonic in another.

In six cases valvular lesions with cardiac dilatation were present. In four of these the heart-lesions were the cause of death, two died suddenly, in one the valvular lesions were mitral stenosis and aortic thickening, in the other the aortic, mitral, and tricuspid valves were all diseased.

In forty-six cases valvular lesions were present without cardiac hypertrophy or dilatation. In only two of these were heart-lesions the cause of death, in neither of which was death sudden. Lesions of the coronary arteries were present in three cases; in one death was sudden. Thrombi of the heart were present in six cases, death sudden in one.

From this report it will also be seen that the number of deaths due directly to heart-lesions was twenty-six. In nineteen cases death was sudden; number of sudden deaths due to heart-lesions, ten; number of gradual deaths due to heart-lesions, sixteen; number of deaths not due to heart-lesions, sixty-four. Of the nine sudden deaths not due to heart-lesion, four were from cerebral apoplexy, four from uræmic convulsions, and one from croupous laryngitis.

The physical evidences of heart-disease are mainly abnormal sounds and impulses which may accompany a cardiac pulsation. In other words, cardiac murmurs are all-important in the diagnosis of cardiac affections. Still, any one who carefully studies heart-murmurs soon comes to the conclusion that it is not the loudest or roughest murmur that is most significant or dangerous; the mere fact that a murmur is present in a given case tells very little of its true nature. For some time I have regarded those cardiac murmurs which occur independent of other appreciable cardiac changes as comparatively of slight importance; and I am convinced that valvular lesions, developed during the period of the growth of the body, are much sooner fatal than those established for the first time at a later period of life.

The records which I have presented give evidence that cardiac murmurs, unless complicated with cardiac hypertrophy or dilatation, rarely necessitate a bad prognosis. Dr. Gairdner states, in his "Clinical Medicine," that he has a patient with an extensive mitral regurgitation which has existed for thirty years, and as yet shows no tendency to shorten life.

I have a patient, sixty years of age, with extensive aortic regurgitation, who has been under my observation eight years, during which time he has had three attacks of pneumonia, yet to-day he apparently has as good a tenure of life as at any time since I have known him; there are no appreciable signs of dilatation in his case.

The results of my own clinical and pathological observations lead me to the opinion that, so long as cardiac murmurs are not complicated by cardiac hypertrophy and dilatation, with the attending loss of rhythm in the heart's action, our prognosis as regards time is good; but, so soon as the signs of considerable dilatation or hypertrophy are present, a great variety of complications are constantly liable to occur, and the heart-disease itself may terminate the life of the individual.

As Dr. Flint has already stated, aortic regurgitation is usually regarded as the most dangerous form of valvular lesion. Of late, however, it has seemed to me that mitral stenosis is equally dangerous. These *post-mortem* records seem to sustain this opinion. Permit me to name a case in point: A female, about thirty years of age, a few weeks since consulted a physician, having walked about one mile and a half to his office. The physician recognized stenosis of the mitral orifice and intense pulmonary congestion. He ordered her to be conveyed to Bellevue Hospital; she, however, walked there, a distance of nearly two miles, and died in about fifteen minutes after reaching the hospital. The only heart-lesion found at the autopsy was mitral stenosis (one finger could hardly be forced through the mitral orifice), and great dilatation of the left auricle. She evidently died of acute pulmonary congestion and œdema, caused by the cardiac lesion and developed by her long walk.

This variety of valvular lesion, it seems to me, has not received sufficient attention. To Dr. Flint is due the credit of first calling attention to it in this city.

Dr. Metcalfe remarked that Dr. Flint's paper had almost exhausted the subject (though by no means the audience), and left little to say except by way of assent and confirmation. But a matter of so great importance would justify some degree of iteration.

It was of the utmost consequence to secure accuracy in diagnosis; and too many of our statistics of the results of heart-diseases were rendered valueless from ignorance or carelessness in regard to this point. Most physicians in their early practice would be sure to fall into errors. He had himself, when he began teaching, been certain of a great many things which he was now convinced that he neither then knew nor had since acquired. A single examination was rarely sufficient to determine the morbid condition beyond a doubt. The patient must be seen under a great variety of circumstances; and care must be taken to eliminate such sources of error as, for instance, the spanæmia of pregnancy and lactation, or the three *isms* of which he used to speak to his class—the nicotism, the rheumatism, and the onanism.

Supposing the diagnosis accurate, the consideration of idiosyncrasy should enter as an important element into the prognosis—the fact that, with apparently the same lesion, one man might live forty years, and another die in as many days. That sudden death from heart-disease was very rare, he was fully convinced. It was the suddenness of a fatal event that patients were most in dread of; and, if they could only be disabused of this idea, their disease would be disarmed of its chief terrors. Ten years ago he had come in charge of a patient whose former physician had diagnosticated extensive lesions of the heart, with atheroma of the aorta. The man was in a state of mortal apprehension; he never left the house without an attendant to watch him; and then his wife would be in an agony of suspense till his return. The speaker had recognized the serious condition of the heart, and told the patient: "You have heart-disease, and I cannot say when you are likely to die; but you are not likely to die suddenly, and my advice is, to dismiss your watchman, and go about your business." The patient had followed his counsel, was still at his business, and was getting along very comfortably, though of course nearer death than ten years ago.

Perhaps the most important of the complications affecting prognosis was renal disease. He knew a gay youth of some sixty summers who had contracted a cardiac affection from rheumatism forty years ago. But his kidneys were in excellent order, and his long-continued good condition was doubtless referrible to the cause insisted upon by Dr. Flint—the power of healthy blood to maintain healthy nutrition, despite considerable derangement of the central organ of circulation.

The doctor related cases bearing upon the subject of angina pectoris, referred to in the paper. A lady whom he had found almost pulseless from this affection, ten years ago, was still alive and doing well. A man whose pulse had once fallen to thirty, and even to twenty-three, in the minute, was likely to last a good while yet. A lady, seven years since, received a blow on the head, which was followed by pneumonia. Her pulse went down to thirty. Mitral disease was diagnosticated;

but she recovered, and last year had another attack of pneumonia, during which the pulse and respiration were each thirty. By April she was convalescent, and had since been very well. Two young ladies, in whose cases his diagnosis of heart-trouble was confirmed by Drs. Swett and Clark, had since married and borne children, and were in a state of health fairly enviable.

THE PRESIDENT hoped that some gentleman would allude to the occasional disappearance of well-marked organic disease of the heart, and the possible clashing of diagnosis which might result. He thought the life-insurance tables, as a rule, bore too hard upon heart-disease.

DR. METCALFE replied that this consideration made yet more evident the necessity of repeated examinations. He could recall many cases where murmurs had existed and then disappeared for a long time, the autopsy finally showing organic lesion. As a life-insurance examiner, he had made it a point, whenever there was any thing wrong in the rhythm or sounds of the heart, never to be satisfied until he had interrogated them under many different conditions. But it was just as important to interrogate the kidneys. He had seen many patients examined for days and days to ascertain the state of the heart, when that of the kidneys was not at all investigated. Only this morning an insurance document had been shown him with the instruction: "*If the policy exceed twenty thousand dollars*, examine the state of the urine."

DR. BIBBINS, speaking of calcification of the heart in the aged, said that for the fourteen years during which he had been physician to the Twentieth Street Asylum for Old Ladies—none of them admitted under sixty years of age, and some of them living to ninety—there had not been a single case where heart-disease had caused sudden death. Such patients died of dropsy, or other more or less protracted illness.

DR. NEFTEL closed the discussion, referring especially to the effect upon the heart of disorder of the vaso-motor nerves, and to angina pectoris as produced by an abnormal condition of the cervical sympathetic.

The Society adjourned.

Bibliographical and Literary Notes.

No subject is of greater importance to the practical physician or surgeon than that of urine-analysis; and though very few can find the time or possess the facilities for conducting the various processes necessary for the solution of the problems in diagnosis which the analytical examination of the urine[1] enables us to solve, it is a satisfaction to know that, in this country as well as in Europe, there are skilful and scientific members of the profession, whose services in the many branches of pathological chemistry and microscopy can readily be obtained. Several years ago Dr. Lionel Beale, Professor of Physiology in King's College, established a Pathological Laboratory, in which analyses of urine and morbid formations generally were conducted with marked success, and we are glad that so thorough and able a worker as Dr. Flint has done the same in New York.

The little volume before us is a marked improvement upon all which have preceded it on the special subject to which it is devoted. The processes are simple, and are described in perspicuous language. The tables with which it abounds very greatly facilitate inquiry. It is a work which ought to be in the hands of every physician and student.

Those who consult this book,[2] with the expectation of finding any infallible recipes either for forming or preserving personal beauty, will be most woefully mistaken; but those who would learn the physiology and hygiene of personal beauty, if we may so express ourselves, may study the volume with advantage. We say the physiology and hygiene, for practically the only secret of preserving personal beauty consists in maintaining the body in its most perfect condition and its most favorable surroundings. And this, we take it, is the real purport of the book before us, and this will be the only good it will accomplish, barring, perhaps, a not unimportant item in that it will inform the uninitiated of the deleterious effects of many if not the most of cosmetic compounds put on the market by unscrupulous and many times unscientific hands. And substantially this same view is admitted by the authors, for in the very opening chapter there is a sort of confession of judgment at inability to do more, and there is also, it seems to us, almost a studied apology for bringing professional acquirements to bear upon what may be deemed by many an unworthy or unprofitable task. To the "girl of the period" the reading of this volume will be as disappointing as its title is enticing. But they who love a genial style, embellished with curious and piquant anecdote culled from a wide range of literature, and the whole pointing a sound moral, drawn from much good advice, will derive profit and pleasure from its pages; and if she—the aforesaid "girl of the period"—could only be induced to heed the teachings therein contained, she would be both a wiser, better, and more useful creature, than now.

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[1] A Manual of Chemical Examination of the Urine in Disease, with Brief Directions for the Examination of the Most Common Varieties of Urinary Calculi. By Austin Flint, Jr., M. D., Professor of Physiology and Microscopy in the Bellevue Hospital Medical College, etc., etc. New York: D. Appleton & Co., 1870. 12mo, pp. 76.

[2] Personal Beauty: How to cultivate and preserve it in Accordance with the Laws of Health. By D. G. Brinton, M. D., and George H. Napheys, M. D. Springfield, Mass.: W. J. Holland, 1870. 12mo, pp. 346.

A STUDIED perusal of this little book[1] has satisfied us of its eminently practical value. The object of the work, the author says, in his preface, is to give the student a few brief and practical directions respecting the management of ordinary cases of labor; and also to point out to him in extraordinary cases when and how he may act upon his own responsibility, and when he ought to send for assistance. With this in view, the management of ordinary labor is first described, and then in Parts II. and III. are consecutively considered the cases which the student may undertake without assistance, and those in which he should ask for counsel. Part IV., an addition by the American editor, is divided into two chapters, devoted to "the Care of the New-born Infant," and "Abortions."

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[1] Obstetric Aphorisms, for the Use of Students commencing Midwifery Practice. By Joseph Griffiths Swayne, M. D. From the fourth revised English edition, with Additions, by Edward R. Hutchins, M. D. Philadelphia: Henry C. Lea, 1870. 16mo, pp. 177.

The work is in the main judiciously done, and it will be received as an acceptable addition to the rapidly-increasing list of books prepared for the especial use of students. If used in accordance with the intent of the author, it can only be serviceable; but if, as is too apt to be the case among American students, in their unseemly haste to acquire their medical education, this book be made to supplant systematic and thorough study of the science and art of midwifery, we should only regret its publication.

On several points we might take exceptions to the teachings of this book; but will specify only two, which happen to occur on the same page (48). The author's respect probably for authority has led him to give his adherence to the old starvation-diet of the lying-in-woman. It was our good fortune, we think, to be taught differently, and in practice we have seen nothing but the best results from precisely the opposite course. To sustain your patients liberally is, we believe, the surest means of warding off many of the complications incident to the *post-partum* state. On the same page the American editor advises never to apply a napkin over the vulva to absorb the lochial discharges, for the reason that "such an application but dams up the lochial discharges against a uterus already excited." Theoretically, granted, provided that napkins be made impervious to fluids, and be fitted to the vulva after the manner of a water-tight joint. Practically the advice is absurd, and the danger purely hypothetical. Carelessness in reading has led to the introduction of several perplexing typographical errors, that mar the perfectness of the volume.

ISSUED in sumptuous style, we have here[1] a reprint, from the first volume of the *Bellevue and Charity Hospital Reports*, of Dr. Taylor's monograph on "Amputation of the Cervix Uteri in certain Forms of Procidentia, with Remarks on Eversion of the Cervix Uteri." The paper is an exceedingly interesting and valuable contribution to gynecological literature, and its value is enhanced by the rare opportunities which the author has had for observing and treating the class of affections named in the title of the article. Premising that, by the term procidentia used in this paper, Dr. Taylor means only that extreme degree of falling in which the uterus has escaped from the vulva, and therefore the vagina is turned more or less completely inside out, and embraces not only the uterus and its appendages, but also the bladder, part of the rectum, and frequently portions of the small intestine, we may state that his experience now reaches eighty-four cases; of these, thirty-seven have been operated upon with thirty-four favorable results.

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[1] On Amputation of the Cervix Uteri in certain Forms of Procidentia, and on Complete Eversion of the Cervix Uteri. By Isaac E. Taylor, M. D., President and Emeritus Professor of Obstetrics and Diseases of Women and Children in the Bellevue Hospital Medical College, etc. New York: D. Appleton & Co., 1869. Quarto, pp. 69, with colored plate.

The conclusions at which Dr. Taylor arrives are so complete an abstract of the paper that it is quite unnecessary for us to do more than reproduce them here. On the points of the pathological conditions existing in the uterine and surrounding tissues in this lesion, and on the history and bibliography of the various operative procedures which have been devised for the relief of procidentia, the paper is unusually full, and must stand as an authority. In this JOURNAL for January, 1869, p. 520, *et seq.*, will be found also a full history of these various operations, which the reader will observe agrees very substantially with Dr. Taylor's researches into this question:

1. That the opinion of M. Huguier—that the affection designated under the names of prolapsus or procidentia of the uterus, and which appears to be completely out of the pelvis, and is exterior to the vulva, is rare—is correct, though not as frequent as M. Huguier supposed, being in the proportion, according to my own investigations, as 1 to $12\frac{5}{6}$, instead of 1 to 32 of M. Huguier.

2. That there exists very seldom a true hypertrophic elongation or pathological change of structure of the supra-vaginal portion of the cervix; but that there is an elongation which, in a great many cases varies from 1 to 4, 5, and 6 inches, though usually $4\frac{1}{2}$ to 5.

3. That the elongation is principally in the *isthmus* or in-

termediate part of the supra-vaginal portion of the cervix, and that this elongation is aided and sustained by the gravity of the cervix consequent, in a great measure, on the changes which have occurred during gestation or parturition.

4. That the assertion of M. Huguier—that the fundus of the uterus remains in the pelvic cavity as high as the superior strait or superior part of the symphysis pubis generally—is not verified; but that the fundus or body of the uterus is usually found retroverted or retroflexed, with the cervix in part external, or, as it is not infrequent, the uterus is procident, and retroflexed *in toto* externally.

5. That the infra-vaginal portion of the cervix is sometimes hypertrophied; but that it is in many instances a true and *complete eversion* of this part, measuring from 2½ to 3½ inches.

6. That it is not necessary to remove as large a conical part of the cervix as described by M. Huguier; but the simple circular method will, in some cases, suffice, though, in other cases, the adoption of the other methods, as proposed, may be resorted to, according to the nature of the case.

7. That the only operation which fulfils the principal and correct indications in *this affection*, for the radical cure of this affection, is the amputation of the cervix uteri.

8. That the contraindications of M. Huguier, which have been referred to, do not forbid the operation, but require it.

9. That, to obtain a more perfect success in the treatment, the operation of episio-perineoraphy should be performed.

A collection of prescriptions used by some of the prominent physicians of the day, as well as many selected from recent publications, has been arranged nosologically by Dr. Napheys.[1] He claims for his compilation the merit of novelty in object and distribution, it containing, not merely "recent formulæ," but specific therapeutical directions, and, to some extent, the philosophy thereof in the management of disease. The book has been carefully prepared, and with fair judgment, and may be recommended; but the title "Modern Thera peutics" should be suppressed, as misleading.

The influences which affect the physical and moral worth of individuals and races, and lead to their improvement or deterioration, are discussed by Dr. Elam, in "A Physician's Problems."[1] The medical man is as much interested in ascertaining their conditions and laws as the sociologist, and to him we commend this book. The design of the author is "to indicate the origin and mode of perpetuation of those varieties of organization, intelligence, and general tendencies toward vice or virtue which seem, on a superficial view, to be so irregularly and capriciously developed, and distributed in families, and among mankind. Subsidiarily, they point to causes for the infinitely varied forms of disorder of nerve and brain, organic and functional, far deeper than those generally believed in; causes that are closely, if not inextricably, connected with our original nature on the one hand, and on the other with our social and political regulations."

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[1] Modern Therapeutics. A Compendium of Recent Formulæ and Specific Therapeutic Directions. By George H. Napheys, A. M., M. D. Philadelphia: S. W. Butler, M. D., 1870. 12mo, pp. 390.

Problem first treats of Natural Heritage, or the essential nature of the permanent and inalienable gifts, material, moral, and intellectual, we derive from our parents, which determine constitution and temperament, and proclivities to health or disease, to virtue or vice, to imbecility or intellect; how far, in fact, an individual represents the organization of his ancestry. The various and recognized influences to which man is liable in the development of his organization and character, and which have a powerful effect for good or ill on his constitution, are intelligently and instructively examined in the second Problem, entitled Degenerations in Man. Much that will attract and inform is to be found under the heads of Body and Mind, Illusions and Hallucinations, Somnambulism, Reverie and Abstraction. If there is nothing very novel, either in thought or matter, in these chapters, the stock-cases are well told, and appropriately introduced, making it a very readable, and, we think, useful compend of the several subjects of which it treats.

The last volume of the Transactions of the American Medical Association[2] is big in bulk, but thin of substance. There is the annual ethical homily, specialism and specialists being the theme this year. The treatment of the subject is hardly more happy than on former occasions. If the Association must erect itself into a high court of professional morals, will it not look at home, and see if there are none there who bring scandal about the house, and whose proclaimed professions to the law, while cleverly keeping themselves on its windy side, are but as sounding brass and a tinkling cymbal? As we have before indicated, it would be well to leave these matters in the hands of the State Societies, which are quite competent to deal with them. The purpose of the Association is the advancement of medicine in the several departments; and if it would be true to its origin, and clear in its office, it should set to work seriously to get out of the rut of commonness and triviality in which it has mostly run since its organization, and, as an earnest of honest reform, begin at once to bring up its Transactions to the level of those of the State Societies.

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[1] "A Physician's Problems." By Charles Elam, M. D., F. R. C. P. Boston: Fields, Osgood & Co. 12mo, pp. 400.

[2] The Transactions of the American Medical Association. Instituted 1841, vol. xx. Philadelphia: printed for the Association. 1869. 8vo, pp. 853.

From the publisher we have received the first volume of the *Journal of the Gynecological Society of Boston*. It is very tastefully gotten up and bound in substantial style. On the first appearance of this journal, we confess to a feeling not of disappointment, but rather of sorrow, for its character was hardly what was to be expected from the respected names of its conductors, and we accordingly passed it by in silence. But these objectionable features were not continued, and since then, by the high character of many of its articles, it has more than compensated for its original derelictions, and has won for itself a reputable and honorable place in our medical literature. The able pen of its principal editor, Dr. Storer, makes each number fairly ring with its incisive criticisms and comments on men, women, and things, and his fresh and hearty way of writing and teaching, which abounds in this volume, can arise only from the most positive convictions of duty and of propriety—and must therefore compel from every one a respectful hearing, even if he be not disposed to accept the fulness of these teachings. Our readers will find the *Journal* a most valuable addition to their library, and we are glad to be able now to give it a most hearty commendation.

Butler's Half-Yearly Compendium of Medical Science comes to us for January, 1870, freighted with its usual choice selection of material. This number contains nearly three hundred articles, collected and condensed from seventy-three different journals. About one-quarter of the articles are by American authors.

Zell's Popular Encyclopædia and Universal Dictionary continues to maintain the same high order of excellence that has characterized it from the beginning. It has now reached Part XXVII., extending nearly through the letter G. For those who are unable to procure the larger encyclopædias, this work promises to be the most useful book of reference of any we are acquainted with.

Henry C. Lea & Co., of Philadelphia, announce new editions of Gray's Anatomy, and Chambers on Indigestion, or the Diseases of the Digestive Organs. Also, Renal Diseases; a Clinical Guide to their Diagnosis and Treatment. By W. R. Basham, M. D.; and Heath's Practical Anatomy, edited by W. W. Keen.

The same firm have also in press a Hand-Book of Medical Microscopy. By Joseph G. Richardson, M. D.

The fifth edition of Neubauer and Vogel's Manual on Urine and Urinary Deposits has been translated into French and published in Paris. It gives the chemical and microscopic properties and characteristics of the normal and abnormal elements of the urine; its qualitative and quantitative analysis; description and semeiological importance of pathological alterations. 31 pages. 4 plates, colored.

New editions of Dr. Mathews Duncan's well-known works, "Perimetritis and Parametritis," and "Researches in Obstetrics," are announced.

Dr. Robert Barnes's Obstetric Operations will be issued by the publishers of this Journal in the early part of the month.

Messrs. J. B. Lippincott & Co. have just issued the thirteenth edition of the United States Dispensatory, revised, corrected, and enlarged, and bringing the work up to date. By Profs. Wood and Bache.

Dr. Horace Dobell, of London, has brought out, under the auspices of Trübner & Co., "Reports on the Progress of Practical and Scientific Medicine." The design of the work is to bring together, in the English language, original and independent reports of the progress of medicine in all quarters of the globe. The reports are prepared by distinguished writers resident in the countries which they represent.

Messrs. Judd & Detweiler, of Washington, D. C., announce for publication, in May, the *National Medical Journal*, to be edited by C. C. Cox, M. D., Professor of Anatomy in the Georgetown Medical College. For the present it will be a quarterly. The prospectus of the *Journal* thus sets forth its claims:

"There appears to be a peculiar appropriateness in locating a first-class medical journal in Washington City, not only on account of its central position, and the various important interests clustering in and around it, but because it embraces a good share of professional ability, needing only the stimulus to medico-literary effort which a judicious, liberal, and well-conducted periodical affords.

"While the principal object of the *Journal* will be to originate and collate useful information in the various branches of medical and surgical science, it is not intended to ignore those prominent *specialties* which have enlisted the zeal and skill of not a few able and enlightened American practitioners. These cannot much longer occupy a subordinate place in the academies and journals of the country, and it is proposed, therefore, to encourage their cultivation and practice within prescribed professional limits."

Books and Pamphlets Received.—Valedictory Address delivered at the Annual Commencement of the Hahnemann Medical College, Philadelphia, March 9, 1870. By Prof. C. G. Raue, M. D. Pamphlet, pp. 15. (From the Author.)

Discourse delivered on the Occasion of the Twenty-second Anniversary of the New York Academy of Medicine, November 11, 1869. By Gouverneur M. Smith, M. D. Published by order of the Academy. New York: William Wood & Co. 1870. Pamphlet, pp. 76.

The Physical and Medical Topography, including Vital, Manufacturing, and other Statistics, of the City of Wheeling. By James E. Reeves, M. D., City Health Officer, etc. Printed by order of the City Council. Wheeling, 1870. Pamphlet, pp. 50. (From the Author.)

We have here a paper, which, though purely local in its associations, is of great interest and value. The title indicates clearly the scope of the paper, which is in fact a medical history of the locality described. Whatever tends to affect the duration of the life of peoples, such, for instance, as the influence of their occupations, their physical hygiene, mental and moral surroundings, are clearly a legitimate subject for study by the medical historian, and all communities owe it to themselves to have these studies made by accomplished medical men—for they only are competent to do this work—and to give the results to the public, that they may be thereby benefited. In this way only can the medical history of great nations be written up; and there is in store a noble work for some judicious and well-informed writer to make up, from just such contributions as this, the vital statistics and medical history of the United States. Dr. Reeves for his own city and vicinity has done this work most admirably, and the City Council have displayed a wise liberality in making public the results of his labors.

Report of the Superintendent of Common Schools of Poweshiek County, Iowa, for the Year 1869. Pamphlet, pp. 20.

Annual Report of the Officers of the Alabama Insane Hospital at Tuscaloosa, for the Year 1869.

Second Report of the St. Mary's Hospital of Philadelphia, for the Year 1869.

The Normal Principles of Education. By John C. Harkness, A. M., President of the State Normal University, Wilmington, Delaware. Pamphlet, pp. 20.

Twelfth Annual Report of the Chicago Charitable Eye and Ear Infirmary, for the Year 1869.

Absence of the Vagina. Three Operations. Establishment of the Menstrual Flow. By M. A. Pallen, M. D. Pamphlet. Reprint from St. Louis Medical and Surgical Journal, pp. 10. (From the Author.)

Valedictory Address to the Graduating Class of Jefferson Medical College, at the Forty-fifth Annual Commencement, March 12, 1870. By J. Aitken Meigs, M. D., Professor of the Institutes of Medicine. Pamphlet, pp. 29.

Valedictory Address delivered before the Graduated Class of the National Medical College, Washington, D. C., March 2, 1870. By John Ordronaux, M. D., Professor of Physiology and Medical Jurisprudence.

Miscellaneous and Scientific Notes.

ADDRESS delivered before the O. Æ. Society of the Bellevue Hospital Medical College, February 24, 1870. By Prof. George T. Elliot, M. D.

Who does not recall with horror the story of the ancient sphinx who propounded riddles and devoured the unfortunate beings who failed in their solution? In modern times we see a revival in another form of the same mystic organization. Symbolized in the enigmatical symbols of the O. Æ. Society, it selects the professors of this college for its victims, and calls upon them to solve the riddles which itself propounds. Thus the unfortunate who addresses you appears at their instance to answer at their bidding to the theme of their own proposing, viz., "Medical Science at the Present Time."

Unlike, however, its great prototype, this modern sphinx, by some weird fascination, compels the attendance of an audience, and rows of sympathetic faces are forced to suffer the inflictions of those addresses which fall in lugubrious tones from the lips of its victims.

Modern physiologists are seeking to establish the locality of the organ of speech. After sailing on the sea of conjecture, and wandering around the convolutions of the cerebral lobes, they have sighted at last those islands of Reil where silver speech is enthroned over golden silence. From these blessed islands come those crops of verbiage which blossom with the thoughts of successive generations, and distinguish man from his fellow-animals.

Now, is it fair that we, who, for six long months, have toiled in these islands for the instruction of this throng of students, should not be permitted to leave them fallow when at last the blessed commencement approaches? No islands will stand such tyranny. Take warning, O. Æ., by what has happened in the ever-faithful isle of Cuba. The typical Alma Mater who nourishes her students from her bounteous breast was but the faint allegory of the teachers who feed their classes from their own cerebral matter. And, after four hundred and forty students have pecked at one for all these weary weeks, how can an audience expect to find a residuum? The islands of Reil are devastated and bare.

What is the state of medicine at the present time? Let me first do homage to the past. Better men than those whose thoughts are embodied in the literature of the past, in languages that live only in science, or in the quaint black-letter which keeps the landmark of the shifting languages of to-day, live not *now*, nor, with even the aid of Darwin, shall illustrate the future. The masterly portraits of disease drawn by those old masters have challenged the admiration of generations, and, like all that is truly great, attract most powerfully the critical admiration of the expert, while, with Cleopatra, they defy that "custom shall stale their infinite variety!" And in their volumes stand expressed those thoughts, unchanged by the gathered learning of centuries, which tell of the hopelessness of exhausting knowledge, and teach the mortal an increasing reverence for Omniscience.

Among the many characteristics of the medicine of to-day there stand in relief the study of the causation and the prevention of disease, with increasing interest in its natural history. And modern medicine marches to its conquests with instruments which rival in perfection those which modern science has given to war. The territory is understood as it never was before. Topographical and regional anatomy are mastered, while comparative anatomy is unfolding to us in the homologies of structure the additional proofs of a grand unity in design. And now physiological anatomy, with its twin-brother physiological chemistry, leads the exploring parties; and the silent workings of the ultimate cells are described, grouped, and classified. We see the long rows of columnar epithelia, with their cilia waving with the uniform and regular sweep of the rows of oars in the triremes of old; we watch the workings of repair of tissue in accordance with the predestined plan, and we predict the aberrations which different conditions compel. The minute anatomy of the kidney is accomplished, and the liver is laid bare to its ultimate fibre; and we see in the individual hepatic cell the escape of freshly-formed bile into the surrounding and ultimate biliary ducts. The nervous system is gradually yielding a knowledge of its mechanism to the systematic advance of scientific exploration, and in the applications of electricity we behold some of the marvels of modern medicine. We see the wasted muscles grow and develop under the judicious application of the constant current; we see the nervous force fed by this mysterious and imponderable agent. The laws of the sympathetic nerve are being unfolded, and the aberrations of its functions explained in familiar experiments, while we influence it at will with our galvanic batteries, and prove our control by watching through the windows of the eye its effect on the pupil and the vessels of the fundus oculi. What would Hippocrates and Celsus and Avicenna have given to have seen with their own eyes and *heard* with their own ears these lectures and those experiments which you have so often yawned over, and so often cut. Imagine

Harvey's delight at seeing the phenomena of the circulation in the capillaries brought under his observation by the microscope, at the transformation of color in the blood in relation to the activity or quiescence of secreting glands! Summon, in imagination, the fathers in medicine, to whom we owe so much, and let them listen to the explanation of the modern views of the anatomy of the eye, and see how the phenomena of accommodation whereby the eagle regulates his rapid swoop from the pure empyrean upon his earthly prey, illustrated in the physiological accommodation of the human eye; let them hear how the perception of color is localized in the rods and cones of the retina, and how the diameter of some of these stands in mysterious and exact measurement to that of waves of light; let them watch in the ophthalmoscope the review of the internal organs of the eye, and let them see the diagnosis made, with that perfection that only the ophthalmoscope can often make, of certain conditions of the brain, of certain diseases of remote organs. Let them see the victim of astigmatism, who has groped purblind through half a lifetime, proclaim the ecstasy with which he for the first time sees things as they really are. Let the victim of increasing deafness proclaim the result of immediate restoration to hearing from insufflation of the Eustachian tube, and restoration of the tympanum to its normal relations to the atmosphere. Let them see the sphygmograph doing its patient and truthful work, and recording the measurements of the pulse with telegraphic accuracy, and stamping with indelible distinctness the diagnoses of the practised ear. Let them see the explorations of hidden cavities with the endoscope and other instruments; let them see in plastic surgery the reunion and restoration of parts hopelessly diseased for them; let them see, in pale, waxy casts and chemical tests the explanation of so much anæmia, so many sites of dropsy; let them see the aberrations of fever recorded by the thermometer, the results of fever measured in the excretions; let Hippocratic succussion be compared with modern methods for determining the presence of fluid in the chest; and picture if you can their astonishment at the methods of modern medicine. Alas! our pride may well be rebuked at the thought of what they might have done if they had had the means. See in the medicine of to-day the wonderful recognition of the parasitic origin of so many diseases, and note the struggle to appreciate the atmospheric germs by which infection may be produced. Watch the grand development of sanitary science in which medicine labors for the protection of society from the preventible causes of disease and death, and see in its inspiration and applications the true nobility of our profession in its highest relations to humanity. Note the daily use of those sedatives and anæsthetics whereby the great curse of physical pain is alleviated and made tolerable for humanity, and see how through the aid of medicine dumb animals are so often brought within the sphere of its beneficent influences. See, in the present improved methods of manufacturing oxygen gas, this great boon to humanity is being brought within the ready reach of medicine. Think of the priceless value of this agent in combating or soothing that tendency to death—by asphyxia—which in many phases may soon be numbered among the surely preventible causes of death. Imagine its influence as a general tonic when it will be introduced into cities, and public buildings, and private houses, as readily as illuminating gas, for the double purpose of increasing illuminating power, and the far greater purpose of supplying that great desideratum of sanitary science, pure air. Think what would have happened in the night made memorable by the fate of those who perished in the Black Hole of Calcutta, if, through those long and weary hours, a tube had poured this vivifying agent into the midst of the sufferers! The terrible struggles of that awful night would have been exchanged for refreshing sleep. And the time will soon come when society will no longer permit the poisonous and mephitic accumulations of crowded buildings to escape the blessed and invigorating influences of this long-misunderstood and much-derided agent, brought at last, through the aid of chemistry, cheaper methods, and the exertions of Doremus and A. H. Smith, to our homes and bedsides.

The great problem of the day is Therapeusis. This, the earliest field of medicine, is yet to furnish its proudest laurels. It will crown the perfect work. First, in the perfection of science, we must know what we deal with, what is this body which we are called upon to treat, that we may escape the satire of Voltaire, that a phyiscian is one who pours drugs of which he knows little into a body of which he knows less; then we must thoroughly know the natural history of disease, that we may not arrogate for remedies the credit of healing that which healed itself; then we must learn how to ward off the tendencies to disease, that we may save the ship that we pilot from the sunken rocks and shallow channels; and, last of all, we must have therapeusis for that which is inevitable, to cure what may be cured, to palliate what may not be averted. Here, freed at last from that empiricism to which humanity owes, after all, such a debt of gratitude, is the great domain and ultimate tendency of medicine; to grapple at last with that which cannot be averted and must be met, which has no tendencies to recovery, or greater tendencies to evil than for good. The full effulgence of this day will never dawn for us, but the future physician may, perhaps, do us that justice which we do to our predecessors, and acknowledge that in faith and hope we see the promised land, and that we are grateful for the great advances of our own day. For my own part, my faith is great. I believe firmly that the beneficent Creator has spread in the planet which we inhabit the remedies for the cure of what in His will is curable, for the relief of that which in His will is to be palliated; that as where the rattlesnake lives there grows the plant which antidotes its venom, so in the sparkling fountains or streamlets, in the ores, the flora, or the fauna of the world, there yet slumber sources of vitality and relief for physical suffering before which the triumphs of the present and past *materia medica* may fade, perhaps, into comparative insignificance. There is need of more concerted action throughout the world in the study of new as well as of approved medical agents. The influences of custom, tradition, and accepted theories sometimes dominate after the solid basis of their power has crumbled before the advance of science. It is, moreover, in human nature to shrink from the polluted touch even when the hand brings guerdon of value; and the mind long familiar with the tricks and devices of charlatanry may wisely doubt the existence of even a grain of wheat in the bushel of chaff. From all these combined influences the march of modern therapeusis halts often, and advances slowly though surely. Be it yours to uphold its banner, and through good and evil report to struggle for the advance of our art, for the welfare of *alma mater*, for the honor of our profession, for the interests of humanity!

WHAT NEXT?—At a meeting of the Odontographic Society of Philadelphia, December 1, 1869, Charles E. Pike, D. D. S., read a paper on "Chloride of Sodium as a Condiment," in which he said: "While I would not wish to be understood as believing that common salt is the sole cause, I *do* believe that it is *one* of the giant evils that are tending so remarkedly to undermine the health of man, and, with the health, the mind."

As pertinent to this matter, and highly corroborative of Dr. Pike's views, we may add that at the recent Temperance Convention, held at Buffalo, in this State, a lady, with that charming reliance upon illogical reasoning which is so characteristic of the female sex, stoutly maintained that the long category of sin and crime which is so rife in the world to-day may be traced directly to the immoderate use of mustard.

Cheap Diplomas.—One A. J. Hale, M. D., of Philadelphia, is issuing a circular to the effect that through his agency he will procure for physicians, lawyers, and clergymen, the degrees of A. M., M. D., LL. D., S. T. D., etc.

Dr. W. P. Seymour, of Troy, New York, has been appointed Professor of Obstetrics in the Albany Medical College. Drs. E. R. Peaslee and Meredith Clymer, of this city, have also accepted chairs in this institution, the former to lecture upon Diseases of Women, and the latter on Diseases of the Nervous System.

Dr. H. L. Hodge, Jr., has been appointed Demonstrator of Anatomy in the University of Pennsylvania, *vice* Dr. D. H. Agnew, who has resigned, and has been invited to the chair of Clinical and Operative Surgery, just created.

Prof. Nathan R. Smith.—This eminent surgeon has resigned the chair of Surgery in the University of Maryland, which he has filled with great ability for the past forty years.

Obituary Record.—We have to record the death at Lexington, Ky., on the 20th of January last, of Benjamin Winslow Dudley, M. D., at the mature age of eighty-five years. Dr. Dudley was one of the most eminent surgeons of his day, and was conceded to be the ablest surgeon in the Mississippi Valley. His great reputation attracted to him most of the operative surgery of that extensive region, and his success, especially as a lithotomist, was remarkable. For many years he filled the two chairs of Anatomy and Surgery in the Medical Department of Transylvania University, and his ability as a teacher of those branches largely contributed to the success of the school. He did not contribute much to the literature of our science, but the few papers he has published make us regret that he did not write more. In his social relations he was universally esteemed for his great worth and public spirit, which secured to him many steadfast friends.

Note on Scarlatina.—Scarlatina will probably increase during the present month, and will afterward decline as warm weather approaches. Some English writers contend (and their ideas are repeated in this country) that scarlatina is a highly-contagious disease, and we are treated with special and minute directions for the disinfection of rooms where the disease has been present, and are urged to give particular care to the disinfection of the clothing and excreta of scarlatina patients. It would be supposed from the directions given that scarlatina is, if possible, more contagious than small-pox. It seems to me that all such teachings in relation to scarlatina are not only erroneous, but are calculated to do very great harm in the community by exciting unnecessary fears, and making unnecessary and utterly useless trouble. In my opinion, scarlatina is not, in any correct sense, a contagious disease; nor is it even infectious in so great a degree as typhoid fever. It is true that our positive knowledge of the causes of scarlatina is very limited. A dozen years since, one of my reports contained the following:

> Less is known in relation to the causes of scarlatina than in relation to those of almost any other disease, and it seems thus far to baffle all sanitary investigation, and bid defiance to all sanitary precautions. Unlike most other epidemics, it visits equally the city and the country; the solitary farm-house and the thriving village; the hill and the valley; and often exhibits its most terrific power where, judging from the known laws of epidemics, it would be least expected. It knows no distinction of classes, but destroys alike the children of native and of foreign parentage; and carries desolation as often to the homes of the rich as of the poor. It sometimes *seems* to arise from contagion; but again it often appears where contagion is impossible.

This was written in 1858. Since that time I have recorded the deaths of several hundreds of children from scarlatina, with all the particulars of age, sex, parentage, locality, etc., and have carefully studied the facts from year to year, and yet what I have learned of the causes of the disease has been little more than confirmatory of the sentiments then expressed. I think now that it is certain that scarlatina not unfrequently appears when there is no possibility of contagion; that it frequently appears where there is no probability of contagion; and that it very often fails to appear where there is the greatest exposure to contagion, if it exists. In a word, it is certain that all efforts to prevent the disease by seclusion or quarantine have utterly failed.—*Dr. Snow's Health Report for February.*

The *Medical Times and Gazette* announces that the Regius Professor of Clinical and General Medicine in the University of Oxford, England (Prof. Acland), has adopted for future use, as his text-book, Niemeyer's Practice, translated by Drs. Humphreys and Hackley, of this city.

The number of horses consumed as food in Paris, during 1869, was 2,758, which furnished 1,003,200 pounds of meat.

GRADUATES IN MEDICINE FOR 1870.—From the various colleges of the country there have been graduated the following numbers of Doctors in Medicine:

Jefferson Medical College, Philadelphia, Pa.,	163
University of Pennsylvania, Philadelphia, Pa.,	113
Bellevue Hospital Medical College,	140
College of Physicians and Surgeons, New York City,	70
University Medical College, New York City,	62
Toland Medical College, San Francisco,	9
Rush Medical College, Chicago,	69
Buffalo Medical College, Buffalo, N. Y.,	41
Washington University, Baltimore,	48
Massachusetts Medical College, Boston,	39
Albany Medical College, Albany, N. Y.,	28
Miami Medical College, Cincinnati, Ohio,	37
Nashville Medical College, Nashville, Tenn.,	58
University of Louisville, Kentucky,	92
Starling Medical College, Columbus, Ohio,	24
Chicago Medical College,	20

SCIENTIFIC CLAIRVOYANCE.—The following communication, addressed to Prof. George T. Elliot, of this city, after listening to a clinical lecture by him at Bellevue Hospital, has been forwarded to us for publication.

NEW YORK, *December* 24, 1869.

DR. ELLIOT—

MY DEAR SIR: I was much interested in your attempt at scientific clairvoyance, in order to determine the size, sex, position of fœtus *in utero*, size of pelvis, and probabilities of safe delivery, in the case at Bellevue Hospital the other day (last Thursday).

A few weeks ago, a coroner's case exposed the following facts: A magnetic and clairvoyant doctor in Brooklyn diagnosticated a tumor in the abdomen as cancerous, treated it with frictions, thumpings over the abdomen, the application of the galvanic current, the administration of something like cantharides, aided by Indian hemp, juniper-berries, pulsatilla, and other emmenagogues. The woman aborted with a seven-months' child, and died.

The well-known Matthias was a religious impostor, whose real name was Robert Matthews. He was born in Washington County, New York, about 1790, and died in Arkansas.

He first kept a country store; married in 1813, and maintained a fair reputation until 1816, when he failed, and went to reside in New York City. In 1827 he removed to Albany, where he became much excited by the preaching of the Rev. Messrs. Kirk and Finney. He afterward engaged actively in the temperance cause, and, claiming to have received a revelation, took to street-preaching. Failing to accomplish his avowed object of converting Albany, he prophesied its destruction, and fled secretly to Sing Sing. Here he imposed upon one family in particular, and many in general, till, finally, he was arrested and taken to White Plains for trial. On the main road, there was a celebrated rocking-stone, which was so nicely balanced that it would be shaken by the wind, and could easily be moved by the hand. This stone, which had thus rocked for ages, fell as the carriage conveying Matthias to trial at White Plains was passing the spot. It had either worn away its bed, so that it would fall from the slightest impulse, or some of the adherents of Matthias had cut away its base, so that it would fall on the slightest jar, or were concealed behind it, to push it over at the right moment. Still, the coincidence was regarded as extraordinary that Matthias's carriage should have been the first to cause its fall; any passing farm-wagon would have accomplished the same result. This coincidence caused great wonderment among his believers. Some thought that his supernatural powers then passed away; others thought that the world would come to an end if the law injured him. He was acquitted in some way, and went to the city of New York, where he involved a number of respectable families among the victims of his delusions. He was tried and acquitted on a charge of poisoning a wealthy disciple, in whose family he was domesticated. In another family, with the consent of the husband, he played the part of the third Joseph, and attempted to rear a second Messiah. The result proved to be a girl, who, when she had grown to womanhood, became married to a respectable merchant, who, however, either neglected to pay, or defrauded the physician who attended the three children born to him from Matthias's promised Messiah. So that prophetic lascivious clairvoyance is not as good even as that of Dr. Simpson.

In this connection, we may allude to the cause of Joanna Southcote, erroneously called Southgate, on page 28, line 29, vol. iv., of the *Medical Gazette.* She was born in Devonshire, England, in 1750, and died in London in 1814. Until nearly forty years of age, she was a domestic servant, and was for some years a member of the Episcopal Church; but, shortly before promulgating her peculiar notions, became a follower of Wesley. In 1792 she began to attract attention by claiming supernatural powers. She found many followers, over whom her influence appears to have been almost supreme; so that at the time of her death her sect was estimated at 100,000. And, as late as 1857, there were still a number of persons who professed to believe in her. When upward of sixty years old, and unmarried, she announced that she was pregnant, and would give birth to a second Shiloh. Dr. William C. Roberts, quoting Gooch, on the signs of pregnancy, says she was then old, fat, a great eater, lazy, and with a protuberant abdomen. She had the power of simulating fœtal movements in her abdomen so completely, that she deceived Dr. Reeves and several London accoucheurs. Shortly before her death, she expressed the conviction that, "if she was deceived, she had, at all events, been misled by some spirit, good or evil." A *post-mortem* examination disclosed the facts that fat, wind, and dropsy, had been mistaken for pregnancy. Her physician published a pamphlet to prove that she had really been pregnant, but her fœtus had dissolved into water (fat and wind).

About Perkins, we find in APPLETONS' CYCLOPÆDIA, from which some of the above facts about Matthias and Joanna Southcote have been taken, that his name was Elisha; he was born in Norwich, Conn., in January, 1740, and died in New York in 1799. He was educated by his father for the profession of medicine, and began the practice of it in Plainfield, Conn., where he was very successful. About 1796 he invented the metallic tractors, consisting of two instruments, one resembling brass and the other steel, but professedly (or quackishly) of a peculiar composition of metals. They were used chiefly in local inflammations, pains in the head, face, teeth, chest, and in rheumatism, paralysis, and other diseases of a similar character.

In the United States, the faculties of three medical institutions recommended the tractors. In Copenhagen, twelve physicians and surgeons, most of them clinical instructors in the Royal Frederick's Hospital, experimented with them, and gave an opinion in their favor, and gave the new system the name of Perkinism.

In London, a Perkinian institution, under the presidency of Lord Rivers, was established, chiefly for the benefit of the poor. The cases of cures published numbered 5,000, and were certified to by eight professors of medicine, forty physicians and surgeons, and thirty clergymen. The list of persons claimed to have been cured by the tractors amounted to an almost fabulous number. But when exposed by Drs. Haygarth and Falconer, of Bath, the tractors fell into neglect almost as speedily as they had become celebrated.

The fate of Perkins is interesting. He invented an antiseptic medicine, and, anxious to test its efficacy against yellow fever, he went to New York in 1799, during the great epidemic there; and, after four weeks of unremitting toil, fell a victim to the disease, in spite of his antidote.

Matthias was born in New York in 1790, and died in Arkansas in 1833, aged 43.

Joanna Southcote was born in Devonshire, England, in 1750, and died in London in 1814, aged 64.

Elisha Perkins was born in Norwich, Conn., in 1740, and visited London; died in New York in 1799, aged 59.

Hahnemann was born in Germany in 1755, and died in Paris in 1843, aged 88.

Joanna Southcote's delusion lasted with some people till 1857; Hahnemann's still lasts in 1870. Matthias and Perkins's delusions have long since died out, except as matters of warning and history.

Very respectfully yours,
JOHN C. PETERS, M. D.

To the Editor of the NEW YORK MEDICAL JOURNAL.

DEAR DOCTOR: If you have room for a brief statement which may call the attention of some to a remedy, not in as general use as it should be, in cases wherein much immediate relief may be thereby obtained, please insert the following facts:

1. After an exposure to cold, I suffered greatly from what is popularly called "stiff neck," which grew so painful as to make it difficult for me to turn my head. It occurred to me to make a trial of my Kidder's battery, and the full force of the interrupted current was applied to the trapezius, which was made to contract vigorously. The relief was very marked and immediate.

2. A patient of mine has suffered from attacks of gout for thirty years. These have been less frequent during the ten years while he has been under my care. He has lately been seized with a severe attack in both feet. For the relief of the pain I have applied Kidder's battery, with moistened sponges. The relief has been so great that he declares that he never desires to be away from such a battery again. His clinical observation embraces the trial of many remedies and springs, and a thorough trial of homœopathy. At times in the night he has removed the fomentations, applied the sponges, and in fifteen minutes or less has been enabled to fall into a quiet

sleep. The advantages of such relief over that obtained by anodynes is apparent.

I may add that a medical friend of mine, subject to a form of gouty neuralgia in his feet, after consuming much lithia and Vichy water, when he had finally experienced the relief from the battery, declares that he is never satisfied to dine anywhere where there is no battery.

Yours, truly,
GEO. T. ELLIOT, M. D.

March 16, 1870.

DEAR DR. DUNSTER:

In the NEW YORK MEDICAL JOURNAL, vol. xi., No. 2, in the paper of Dr. Lente it is stated (p. 108) that I "first actually suggested the use of the hypodermic method publicly in this country, and had the first American instrument manufactured." The doctor refers to the *New York Journal of Medicine*, vol. iv., p. 340, for my first published cases.

Those cases were, as I believe, the first ever published in this country; and it is true that the first instrument ever manufactured in this country was made for me, at my instance, by Tiemann & Co.

Now, in these published cases, I give my friend and colleague, Dr. Barker, the credit of having shown me the first instrument which I had ever seen and used, viz., one that he had brought from Europe himself.

Those who do not refer to the original article will not appreciate this fact, which I therefore, as the subject is one of interest, again mention, in justice to Dr. Barker.

Yours faithfully,
April 7, 1870. GEO. T. ELLIOT.

THE University of Vienna has decided to open its medical lectures to women and confer medical diplomas on them.

MISS ELIZABETH GARRETT, M. D., has been appointed one of the visiting physicians to the East London Children's Hospital. This is the first medical appointment given to a woman in England.

NEW YORK STATE HOSPITAL FOR DISEASES OF THE NERVOUS SYSTEM.—A meeting of the Trustees of this recently-incorporated charity was held at the residence of William H. Appleton, Esq., No. 3 Madison Avenue, on the evening of the 23d inst. The following is a list of the Trustees: William H. Appleton, George T. Elliot, Edward Cooper, Lewis A. Sayre, H. C. Fahnestock, Thurlow Weed, William A. Hammond, Roswell D. Hatch, William H. Van Buren, Frederick S. Winston, James R. Wood, and Austin Flint. The organization of the Board was completed by the unanimous election of the following officers: *President*, William H. Appleton, Esq.; *Vice-President*, Thurlow Weed, Esq.; *Treasurer*, H. C. Fahnestock, Esq.; *Secretary*, Roswell D. Hatch, Esq.

The following medical officers of the Hospital were then unanimously elected: *Physician-in-Chief*, Dr. William A. Hammond; *Consulting Physicians*, Dr. Austin Flint, Dr. Geo. T. Elliot; *Consulting Surgeons*, Dr. James R. Wood, Dr. Lewis A. Sayre.

The Physician-in-Chief was authorized to appoint an Assistant Physician, and Dr. R. A. Vance was named by him for this place. A House Physician and four clinical assistants are to be appointed, after passing a satisfactory examination.

The objects of this Hospital, as explained in the charter, are:

First. To give and furnish gratuitous relief for indigent persons (without regard to nationality or creed) who may be affected with acute or chronic diseases of the nervous system, especially epilepsy and paralysis, but excluding actual insanity.

Second. To afford an opportunity to physicians and medical students for clinical observation and instruction in the special branches of science referred to in the first subdivision —diseases of the brain and nervous system. The Trustees are confident of being able to erect a suitable building shortly. For the present, they will rent one.

Army Personal.—Since October 1, 1869, the date of our last report, the following changes have occurred in the Medical Department of the Army:

Resigned.—Assistant-Surgeon Wm. Smith, to date January 5, 1870. Assistant-Surgeon G. H. T. F. Axt, to date February 1, 1870.

Resignation revoked.—Surgeon H. R. Wirtz, Brevet Lieutenant-Colonel. The special orders accepting the resignation of this officer, to take effect March 1, 1870, revoked by order of the President, per Special Orders No. 24, dated Adjutant-General's Office, January 29, 1870.

Retired.—Brevet Brigadier-General R. S. Satterlee, Chief Medical Purveyor, U. S. Army. By order of the President, retired from active service, to take effect February 22, 1869.

Under date of March 9, 1869, by direction of the President, ordered to remain on duty as Chief Medical Purveyor in New York City. Relieved from duty February 21, 1870, by the provisions of Par. 1, General Orders No. 15, dated Adjutant-General's Office, February 5, 1870.

Brevet Brigadier-General Charles McDougall, Assistant Medical Purveyor, U. S. Army. By order of the President, retired from active service, to take effect February 22, 1869. Under date of March 9, 1869, by direction of the President, ordered to remain on duty as Assistant Medical Purveyor in St. Louis, Mo. Relieved from duty February 21, 1870, by the provisions of Par. 1, General Orders No. 15, dated Adjutant-General's Office, February 5, 1870.

LIBERAL GIFT TO THE ALBANY MEDICAL COLLEGE.—At a meeting of the Trustees of the Albany Medical College, held, pursuant to notice, on the 7th of April, 1870, Thomas W. Olcott, Esq., presented to the Board a communication from Dr. Thomas C. Durant, of New York, expressing the strong and earnest sympathy of the writer with the prosperity and efficiency of the Albany Medical College, and his confidence in the administration, and tendering his hearty coöperation in all measures by which its success might be promoted and its usefulness be unimpaired; and, in consideration of his former connection with the institution as a student, and as an expression of his high esteem of Dr. Armsby and his appreciation of his indefatigable efforts to establish and sustain the college, tendering the liberal donation of $15,000, the interest of which shall be applied to establish a Professorship of Surgery, to be called the "March Professorship," in memory of the late Dr. Alden March, such interest to be so applied so long as the regular annual course of lectures shall be continued, and submitting his said proposition to the action of the Board of Trustees. On motion, it was

Resolved, That the Trustees of the Albany Medical College do acknowledge with pleasure the expression of kindly interest in the College, and accept with gratitude the munificent testimonial with which Dr. Durant so practically asserts the sincerity of his interest.

Resolved, That it be referred to a committee, consisting of Judge Harris, Judge Parker, and Thomas W. Olcott, to take such further action in the premises as may be necessary to give legality to the trust, and permanency and efficiency to the endowment so generously tendered by Dr. Durant.

Resolved, That the thanks of this Board be tendered to Dr. Thomas C. Durant, as well for his expression of interest and confidence in the Albany Medical College, as for the generous donation with which such expression is accompanied.

Resolved, That these resolutions be entered on the minutes of the Board, and a copy thereof be transmitted by the Secretary to Dr. Durant, and published in the papers of the city.

GEORGE DEXTER, *Secretary*.

Albany Evening Journal, April 8th.

SKEPTICS AND ANTISEPTICS.—*A propos* of the opposing views held by different authorities on the value of the antiseptic treatment of wounds, the following *jeu d'esprit*, which we find in the *British Medical Journal*, copied from a Scotch paper, will amuse our readers:

I.

Oh! who would be a skeptic?
 Oh! who would dare to rail
At Lister's antiseptic,
 And hint that it may fail?
You ask me what's the *matter*—
 You ask me what's the row.
"Why! *Pus*, that used to scatter
 Disease, is ended now."

II.

We've a new-fangled notion,
 Though not from *Germ*any,
That everywhere in motion
 Germs of disease there be.
Though these we can't discover
 By touch, by sight, by taste,
There they are, and wounds we cover
 With antiseptic paste.

III.

One might *hope* with manner placid
 This doctrine they'd receive—
Adopt carbolic acid,
 Its miracles believe.
But ah! 'tis most distressing,
 There's *ferment* without end,
Which antiseptic dressing
 I fear would scarcely mend.

IV.

For Spence hits hard at Lister—
 Lister lets fly at Spence;
And like a running blister
 They give and take offence.
To keep the peace to bind them—
 Strong silk or e'en catgut
Will not suffice—you'll find them
 Returning cut for cut.

V.

Catgut, by fiddle-scrapers
 Was once monopolized;
Now with carbolic vapors
 Impregnated 'tis prized.
The *tenor* of the invention,
 Spite of reflections *base*,
Is by "the first intention"
 That healing should take place.

VI.

Spence, helped by Simpson often,
 Says catgut plays the deuce—
Maintains that it will soften,
 And swelling become loose.
But Lister effervescent
 Denies that this is true—
"*That* catgut was putrescent
 Which Lawrie sent to you."

VII.

The question still *sub judice*
 Undoubtedly remains—
It affects both me and you, d'ye see—
 Who loses and who gains.
But let it be tried fairly,
 That Lister has a right
To ask, for late and early
 He works with all his might.

VITAL STATISTICS OF RHODE ISLAND.—The Annual Registration Report of Rhode Island, for 1867, to which we have previously alluded, contains much valuable and interesting material. We note a few points only.

Regarding the relative number of the sexes, there were born in 1867, 5,127 children; of these, 2,655 were males, 2,464 females, and eight, sex not stated. Excluding these eight, there were 107.7 males to each 100 females. In the State of Massachusetts for the same year, the proportion of male children born was 104.2 to each 100 female children. For the fourteen years previous to 1867, there were born in Rhode Island 27,379 boys and 25,746 girls, making the proportion of males to females, for this period of time, 106.3 to 100. In Massachusetts, for the same period, it was 105.9 male to each 100 female children.

Studies of statistics in other countries, and on larger scales, give results corresponding very closely with the proportions above stated.

In both Rhode Island and Massachusetts, during this year (1867), the number of children born of foreign parentage was considerably greater than the number born of native parents. The colored people of the State, for seven years (1861–1867), show 690 births against 781 deaths.

Ovariotomy has made its way into Sweden, and will probably ere long be recognized there as a legitimate operation in surgery. Dr. Sköldberg, a young surgeon, who studied but a short time since in London, under Wells, and in Edinburgh under Keith, returning to his native country, has now performed the operation twenty-one times, with the result of seventeen recoveries and four deaths. In one of the fatal cases the tumor was malignant. In four cases he performed exploratory incision, and one of these died of peritonitis in fifty hours after the operation.

The Man who may be expected to live long.—He has a proper and well-proportioned stature, without, however, being too tall. He is rather of the middle size, and somewhat thick set. His complexion is not too florid; at any rate, too much ruddiness in youth is seldom a sign of longevity. His hair approaches rather to the fair than to the black. His skin is strong, but not rough. His head is not too big; he has large veins in the extremities; his shoulders are round rather than flat. His neck is not too long; his abdomen does not project; his hands are large, but not deeply cleft. His foot is rather thick than long; and his legs are firm and round. He has a broad, arched chest, a strong voice, and the faculty of retaining his breath for a long time without difficulty. There is harmony in all his parts. His senses are good, but not too delicate; his pulse is slow and regular. His stomach is excellent; his appetite good, and digestion easy. The joys of the table are not to him of importance; they tune his mind to serenity, and his soul partakes in the pleasure which they communicate. He does not eat merely for the sake of eating, but each meal is an hour of daily festivity. He eats slowly, and has not too much thirst—the latter being always a sign of rapid self-consumption. He is serene, loquacious, active, susceptible of joy, love, and hope, but insensible to the impressions of hatred, anger, and avarice. His passions never become violent or destructive. If ever he gives way to anger, he experiences rather a useful glow of warmth, an artificial and gentle fever, without an overflowing of the bile. He is fond also of employment, particularly calm meditation and agreeable speculations. He is an optimist, a friend to Nature and domestic felicity. He has no thirst after honor or riches, and banishes all thoughts of to-morrow.—*Hufeland.*

PROF. F. G. WELCH, who is Professor of Physical Culture at Yale College, thus fulminates against tobacco in his work on "The Philosophy of True Living," which we noticed in the Journal of February. Since King James's Counterblast, we have seen nothing quite equal to this, unless it be some of the Rev. Mr. Trask's lucubrations. Prof. Welch would have saved time and trouble by at once asserting that tobacco is the cause of all the evil existing in the world; and he would have been not much further from the truth than he now is. We are no advocates of the general use of tobacco, for we know that in many cases it does harm; but Prof. Welch would do well, before attempting any further crusade of this sort, to acquaint himself with the physiological effects of tobacco on the human system, of which it is now plainly evident he knows nothing.

The use of this nauseous and filthy weed we must class among the worst of vulgar excesses. It was discovered by Sir Francis Drake, near Tobasco (Yucatan, Mexico), hence its name. It is one of the greatest, if not the greatest, enemy to physical life. It is the most potent enemy to right moral character. "For what was tobacco made?" is a question often asked, evidently intending to prove that because it is a natural production it is proper and right to use it for chewing, smoking, and snuffing. Opium, deadly night-shade, and henbane, are all natural productions. Should these, therefore, become habitual luxuries? Tobacco is one of the very strongest poisons. A single drop of the concentrated oil put upon the tongue of an animal will destroy its life instantly. It is one of the most unnatural and poisonous things that can be taken into the mouth. An appetite for it is entirely artificial, created by habit. God never made such an appetite, nor made the weed to be used as a luxury. He made man for more elevated enjoyments, for more dignified practices, for more reasonable devotions. It as truly intoxicates the brain and nerves as does alcohol. The smoking of a single cigar increases the pulse from fifteen to twenty beats per minute. It is quackery to prescribe it so often as medicine. When thus prescribed, the mouth is no place for it. It enters the circulation and destroys the healthful properties of the blood. The very worst forms of dyspepsia, with its long train of attendant evils—piles and chronic diarrhœa—are produced by its use. Do not commence! If a man begins taking it, he takes it eternally; he finds no leaving-off place. A crime against Nature is a crime against God. The tobacco-devotee knows his course is wrong. The tobacco-user is giving forth pestilential vapors from all the pores of his skin. He is an embodiment of perpetual miasma; a walking distillery of deadly essence. Tobacco costs every year five times as much as the support of the Gospel. Man is quite sufficiently animal in his character without any such artificial promptings as tobacco, alcohol, or any other producer of unnatural appetite. Tobacco excites the nervous system, and in time renders it prostrate. It has the same effect on the mind. The tobacco-user must necessarily lose his moral courage, self-respect, and self-government. It also enslaves the mind. No man can be strictly a gentleman who uses tobacco in any form. Men are wanting in moral courage who cannot give up the use of this destroyer. Let them combat the foe at every avenue, and be determined to conquer or die! Oh, the power of "I will!" Tobacco blunts the conscience. A writer in one of our periodicals, speaking of the effects of tobacco in his own case, says that "smoking and chewing produced a continual thirst for stimulating drinks; and this tormenting thirst led me into the habit of drinking ale, porter, brandy, and other kinds of spirits, even to the extent of intoxication." The same writer adds that, after he had subdued his appetite for tobacco, he "lost all desire for stimulating drinks." It destroys the teeth, instead of preserving them; wears off the enamel, thereby causing decay, ulcers in the gums, wearing the teeth down, giving a filthy mouth. Its consumers are always benefited by quitting it at once and forever; and it can be, and can be done in this way, or not at all. There is no foe in human society that is so enticing, enslaving, or so invincible. Health and longevity are Christian duties, and their abuse is a crime against Nature and Nature's God. Every man who knowingly brings upon himself disease and death by tobacco, is a suicide; and drunkards and suicides cannot enter the kingdom of heaven. Whosoever wars with Nature, must some time pay the forfeit—the penalty. It destroys health in many ways; the fluids, the solids, the nervous system, the brain, the heart, mind, intellect, morals, are all affected by it. Who can deny it? It destroys eyesight, hearing, and circulation. Every minister of the Gospel, as well as every physician, ought to cry aloud against it; for it destroys the highest susceptibility of the soul. It is as truly a sin to transgress physiological law as to violate one of the Ten Commandments. The tobacco-user's judgment, reason, and common-sense, all conjoin their testimony that this is a sin, yet he heeds it not; it is possible to convince, to convict, but to convert is difficult. It greatly retards the progress of temperance, and is almost certain to lead to intemperance. Tobacco and liquor may be called twin-brothers—nay, rather, twin-devils.

To this manifesto Prof. Welch appends forty-one reasons against the use of tobacco, of which the first is a fair sample. I. It is a main upholder of slavery in the United States.

Successful Extirpation of one Kidney.—Mr. T. Spencer Wells, of London, sends a note to the *Medical Times and Gazette*, giving an extract from a letter received by him, from Prof. Wagner, of Königsberg. This letter mentions briefly a case which had been operated on by Simon, viz.:

"As I passed through Heidelberg, I saw a patient of Simon's whose case will interest you extremely. A provincial surgeon had performed ovariotomy for her; and, on account of inseparable adhesions, had removed both ovaries and the uterus as far as the neck. The patient recovered. It appeared, however, that the operator had also injured the right ureter, for a urethral fistula formed in the cicatrix in the abdominal wall. This led to the patient going to Simon. After several unsuccessful attempts to cure the fistula, Simon, at last, *extirpated the right kidney.* He went in from the lumbar region, shelled the kidney out of its capsule, tied *en masse*, and cut it away. I saw the patient *cured*, and going about with the fistula also cured. The ligature threads had not come away. Simon now proposes extirpation of a kidney for such diseases as echinococcus, abscess, hydronephrosis, and renal calculi."

I may add that I have seen a healthy kidney, inseparably connected with a fibro-cystic tumor of the uterus, removed with the tumor; and, although the patient died on the third day, there was not one symptom which could be referred to the loss of the kidney. The urine was normal in quantity and character, and there was no sign of uræmia.

I have also published cases which show that cysts of the kidney may be very successfully treated by tapping and drainage, and have joined in advising nephrotomy in some cases of renal calculi. When Simon publishes his case of extirpation of the kidney, he will no doubt discuss the principle of this operation, and we shall be glad to have a broad distinction drawn between cases in which disease of one kidney is killing a patient, and such a case as that of a urinary fistula where life is *not* threatened.

An Electrical Infant.—There is a wonderful account in all the French papers of an astounding baby just dead, at the age of ten months, at St. Urbain, near Lyons. The strongest medical evidence is said to be given that the child was so highly endowed with electricity that all the persons in the same room with him received constant electric shocks. Its end was apparently painless, but accompanied by still more astounding manifestations. At the instant of death luminous effluvia proceeded, it is affirmed by the doctors, from the body of the child, which continued for several minutes after its decease. The case is supposed to be quite unprecedented.

A Physician on the Stand.—The standard legal dictionaries in use may be searched in vain for more accurate definitions of legal terms than those recently given by a physician in Pulaski, Tennessee, as reported to us by a leading member of the bar of that place. Mr. B. and a Mr. L., opposing counsel in a pending case, were engaged in taking depositions to be used on the trial of the cause. The question as to a certain woman's soundness of mind being in controversy, a physician was called as a medical expert, and during his examination the following dialogue took place:

Question.—Do you think this lady is of sound mind?

Answer.—No, sir; I do not.

Ques.—Does she know the difference between a "power of attorney" and an "absolute conveyance?"

Ans.—No, sir; of course she don't, and there are very few women who do.

Ques.—Do you know the difference?

Ans.—Yes, sir; of course I do; do you suppose I am an ignoramus?

Ques.—Well, sir, will you be kind enough to tell us the difference?

Ans.—Well—well, a "power of attorney" is the strength of mind of any particular lawyer, and an "absolute conveyance" is a hack, or omnibus, or railroad car, or something of the sort.—*Bench and Bar.*

A child, four years old, accidentally burned all over the body to the third or fourth degree, was recently admitted to the Child's Hospital of Lausanne. On the fourth day after his arrival, the suppuration from his wounds was so abundant and fetid, that the quarter in which he was lodged became uninhabitable, and putrid intoxication was considered imminent. M. Joel then placed him in a bath containing two handfuls of sulphate of iron. The cessation of pain was almost immediate; after repeating this bath twice a day, for fifteen or twenty minutes at a time, the suppuration moderated, the fetid odor disappeared, and the little sufferer recovered rapidly.

THE *London Medical Times and Gazette*, of March 26th, says: "The NEW YORK MEDICAL JOURNAL for March, 1870, contains two capital articles on relapsing fever, which prevails rather extensively in that city. The one is a paper by Dr. Meredith Clymer, who reported the first cases which occurred in the United States, twenty-five years ago. The other consists of two clinical lectures by Dr. Austin Flint, Sen., delivered at the Bellevue Hospital, where one hundred and eight cases have been treated."

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ART. I.—*Intra-Uterine Medication.* By J. C. NOTT, M. D., New York.

IN the May number of the *American Journal of Obstetrics*, etc., I published an article (read before the Medical Society of the County of New York), the object of which was to show that a *chemical reaction* almost invariably takes place between the intra-uterine remedies commonly used and the uterine discharges, whether mucous or sanguineous.

All these discharges being highly albuminous, they combine instantly with most metallic salts and acids thus employed, and such remedies are, as a rule, *neutralized*, and rendered inert by the chemical reaction. For example, persulphate, or other salt of iron, chromic acid, nitrate of silver, etc., when introduced into the cavity of the uterus, are instantly neutralized by the chemical reaction, unless the remedy be *in excess* over the uterine discharge.

I propose now to give some practical hints on intra-uterine medication; but, to make myself clearly understood, I must, at the risk of fatiguing the reader, give a short *résumé* of some points of the normal and morbid anatomy of the uterus.

Anatomists usually describe the uterus as consisting of three coats—the serous, the muscular, and the mucous—but a striking peculiarity is here found which exists nowhere else. There is no sub-serous or sub-mucous tissue, and the three coats are inseparably blended in one mass, so that they cannot be dissected apart. This intimate connection accounts for the great facility with which diseased action is communicated from one tissue to another, and from the uterus to the Fallopian tubes, ovaria, and surrounding tissues.

The walls of the uterus, in its normal state, except during menstruation and pregnancy, lie in such immediate contact that no open cavity exists, and, when distended by injection, the organ does not contain (according to Bennet) more than from *nine to twelve minims.*

The mucous membrane of the uterus differs materially from all other mucous membranes in structure—it forms *a part of the uterine walls*, and makes up one-fifth to one-fourth of its thickness.—*Bennet.*

Weber, Sharpey, Richert, and other authorities, agree that the utricular glands or follicles consist of involutions or depressions of the mucous membrane, which are exceedingly numerous and lie close together. They generally present the form of canals, taking their course through the substance of the parenchyma of the mucous membrane toward its surface, where they terminate each in a separate orifice.

"Whether the follicles terminate in a blind extremity in every case, as Weber represents them, or whether by an *indirect communication with the uterine vessels*, which many considerations both physiological and pathological seem to point out as at least possible, it is difficult to ascertain."—*Cyc. Anat. and Phys.—Arthur Farre.*

"The veins of the uterus," says Bennet, "are remarkable for their great size, which is very much greater than that of the arteries—for their frequent anastomoses—and for their anatomical structure. The *external membrane being absent, the internal membrane is in immediate contact with the proper tissue of the uterus, so that the walls of the veins are contractile.*

"The arrangement of the capillary vessels is peculiar and characteristic. The capillaries, which are of large size, usually descend between the canals of the uterine glands, giving to them a few small branches in their course. Having reached the surface of the mucous membrane, they spread out into a mesh-work of round, oval, and hexagonal spaces, in the centre of each of which may usually be observed the orifice of the uterine gland.

"The net-work of capillaries thus formed lies *very superficially with regard to the uterine surface*—the layer of epithelium covering them and the nuclear corpuscles, and amorphous tissue supporting them, appear to have so little cohesion, and to form so slight a protection, *that the vessels are often seen to be nearly bare*, while in some instances the individual capillaries may be observed *hanging out loose* in the uterine cavity, and giving to its surface a villous appearance."—*Arthur Farre, loc. cit.*

The exposed condition of the uterine circulation, and the peculiar anatomical structure, I shall again allude to as very favorable to the entrance of fluids or air into the general circulation, in diseased conditions of this organ.

Morbid Anatomy.—In addition to the few remarks above on the normal anatomy of the uterus which bear directly on my subject, I propose now to say a few words on its morbid anatomy.

Endometritis is confessedly an obscure subject, its diagnosis difficult, and the most opposite pathological conditions have by most practitioners been grouped under one head and treated by the same remedies. *Endometritis, uterine catarrh, acute and chronic metrorrhœa, leucorrhœa, fluor albus*, are by many used as synonymes, and any mucous, sero-mucous, or muco-purulent discharge from the uterus is too often looked upon as the external manifestation of one hidden disease. This, however, is a great mistake, as we shall proceed to show.

We may assume, at the offstart, that, whatever difference of opinion may exist with regard to the *inflammatory* nature of such affections, there is little as to the pathological identity of simple catarrhs of the uterine and other mucous membranes. The morbid action may be confined to the body of the organ, to the cervix, or may invade both at the same time.

I shall say nothing about two forms of uterine leucorrhœa: 1. The acute inflammatory form; 2. That chronic form which is entirely disconnected from inflammation, and is a mere flux depending on some deranged state of the system. These are both conditions which rarely require *intra-uterine medication.* The first is cured by antiphlogistic, and the second by constitutional treatment, tonics, etc. My remarks are meant to apply more particularly to those diseased conditions (as in other mucous surfaces) where local applications are indicated.

In the chronic forms of simple, uncomplicated uterine catarrh, the pathologist would find after death little to attract attention—the appearance would be that of chronic bronchitis—of vesical catarrh, etc.; the mucous follicles would be found a little swollen and patulous, the membrane a little thickened and softened, the secretion increased and more or less mucopurulent, but no marked organic lesion, and the blood perhaps gone from the turgid capillaries.

But a very different state of the mucous membrane is often seen. In membranous dysmenorrhœa and pregnancy, for example, the mucous membrane is thrown off entire, *utricular follicles and all;* and, what is very important in connection with uterine catarrh, the mucous membrane, after pregnancy, is not *reformed under two months.* Moreover, if a morbid state should follow pregnancy by which chronic engorgement or inflammatory action should be set up, the mucous membrane *does not reform at all*, its surface may continue denuded, its blood-vessels exposed and pouring out unhealthy secretions, thus constituting a bad form of endometritis. Hence it is that so large a proportion of the cases of this disease follow parturition or abortions and that this class is so difficult to cure.

Unfortunately, we have very imperfect means of examining the appearance and progress of diseased action when seated within the cavity of the uterus. But we can see what goes on at the external os, and within the cervix for a short distance, and may fairly judge the part out of sight, by that which we do see.

"In the language of Dr. Farre, when inflammation affects chiefly the cervical mucous membrane, this, in like manner, becomes congested and swollen. The congestion affects more particularly the capillaries of the vaginal portion of the cervix, and of the interior of the canal near the os externum. The lips of the os tincæ become tumid, the os is enlarged, and the whole cervical canal expanded, changes which indicate that the submucous tissue is involved. A loss of epithelium in the neighborhood of the external orifice, more or less extensive, often accompanies the severer forms of this affection. From this it results that the turgid and vascular papillæ beneath become exposed, and, when these are also hypertrophied the surface acquires the condition commonly termed *granular.*

"The natural or healthy secretions of the cervix become materially altered under catarrh. In a normal state the cervical secretion is sufficient in quantity to cover the mucous folds and to fill the crypts and furrows, and occasionally to block up the entire canal. It consists of a viscid, tenacious, and nearly-transparent fluid, enveloping numerous mucous corpuscles, granules, and epithelial scales.

"When the catarrhal state ensues, this fluid is greatly increased in quantity, and, according to the severity of the affection, it passes through the various conditions of a viscid, transparent jelly, resembling clear starch or white of egg—of a thicker cream-like fluid, or a puriform mucus, in color nearly resembling pus."

It is also a pathological fact, and one of importance in connection with what we have to say about intra-uterine medication, that, although healthy mucus is rich in albumen, when its secreting membrane is inflamed and the secretion increased, the *albumen,* as a general rule, *increases* in proportion as the secretion becomes more abnormal in quantity and quality. It is also worthy of remark that mucus not only putrefies readily, but communicates promptly the putrefactive process to other animal fluids.

Another and different pathological condition of the cavity of the uterus is seen in the *formations* which take their origin in its lining membrane. They consist chiefly in hypertrophic growths of that membrane, and of its follicular structures. They present usually two varieties, according as the follicles or the ordinary mucous tissue abounds in their composition.

Many of these growths acquire a peduncle and then constitute the mucous or follicular polypi. They are most common in the body and fundus, and are of the size of a pea or small plum. The follicles are bound together by loose, hypertrophied fibrous tissues. The more solid mucous tumors generally acquire a stem, and early take the form of polypi. They mostly arise between the folds of the lining membrane of the cervix. All these polypoid growths give rise to uterine discharges, and are in their early stages generally thrown into the group of endometritis.

Klob, in speaking of catarrhal inflammation of the body of the uterus, says: "In many cases the glandular utricular follicles cast off their entire cellular coverings, which latter are found in the mucus, as collapsed casts. Nylander and Virchow have observed a similar expulsion of the whole contents of the glands during menstruation, and I have repeatedly seen the same in various tumefactions of the uterine mucous membrane. Finally the color of the secretion changes to yellow or yellowish, and from the admixture of purulent elements it becomes cream-like."

Again, of chronic catarrh of the body, Klob says: "Its surface is either smooth or papillary and uneven—the latter being especially the case at the posterior wall, which is sometimes covered with various secretions, and *growths resembling granulations.* The membrane is also soft and more succulent, but can seldom be separated from the uterine walls in large pieces as in acute catarrh.

"When chronic catarrh is of long standing, the mucous membrane, *especially that of the body and fundus*, undergoes important anatomical changes. Its glands, either from constriction or atrophy of their superior portions, frequently change into small cysts, or are cast off, which latter occurrence gives the mucous membrane a *net-like* appearance.

"In some cases we also notice a *desquamation of the epithelium and erosions*, and small, smooth-lined depressions, evidently formed by the rupture of small cysts. It is probably owing to this development and rupture of cysts, that the delicate ridge-like elevations are formed, especially at the internal orifice, which give rise to adhesions.

"More rarely we find, according to Rokitansky, the mucous membrane transformed into a *callous stratum*, varying in thickness and attached to the submucous tissue, and in this stratum we find small cysts, which are the remains of degenerate glands.

"More frequently the dense submucous stratum, especially at the borders of the internal orifice, becomes atrophied, and Nabothian vesicles are developed in it, thus *causing a predisposition to flexion*."

Tyler Smith denies the frequency, almost the existence of uterine engorgement and leucorrhœa, as connected with the body of the uterus, and regards it almost wholly as a *cervical* disease. Scanzoni and most pathologists of the present day oppose him.

The following *résumé* from West, with the facts before detailed, places the matter in an unanswerable light, to my mind:

"The casting off of the epithelium from the body of the uterus during menstruation—the discharge which sometimes precedes, and often follows, the catamenia—the protracted lochial discharge after confinement—the copious leucorrhœal discharge that often takes place after menstruation, or after confinement—the copious leucorrhœal discharge that often takes the place of menstruation, the leucorrhœal discharge monthly during nursing, the discharges found after death, both in uterus and Fallopian tubes, the profusion of utricular glands in the body, the muco-purulent discharges found distending the uterus where closure of its mouth has occurred, the leucorrhœal discharge which is seen from the inverted organ—all these facts prove the body of the uterus to be a frequent source of catarrhal discharge."

Hennig asserts that catarrh of the Fallopian tubes is more common than similar affections of the body, neck, or vagina, and West says: "It seems doubtful whether affection of the Fallopian tubes does not play a far more important part than we have been accustomed to suppose, in the production of many of the ailments of the female sexual organs."

Having completed the above fragmentary sketch of those portions of normal and morbid anatomy that bear directly on the points to be elucidated, I proceed to give their practical application. And first, a few words about the *internal capacity* of the uterus, and of the behavior of the organ when fluids are injected into it.

In multiparæ the uterus is a fraction larger and more globular than in the virgin, but the difference, in internal capacity, is insignificant. According to Bennet, Courty, and others, the cavity of the normal virgin uterus, when distended, is not more than ten to twelve minims, and when not distended, artificially, or by the menstrual flow, no *open cavity* presents, the two walls being in immediate contact. In uterine catarrh it may be increased to a drachm or two, and, if retroflexion exist, several drachms more. In cases of polypi and fibroids, in menorrhagia, after abortions, and labors, the capacity may be more or less increased.

If a canula or the pipe of a syringe be introduced into the cavity of a normal uterus, it will block up the *cervical canal and occupy one-half of the cavity of the body.* If water then be thrown into the cavity through the tube by forcing down the piston of a syringe, it is evident that considerable dilating force must be exerted on the walls of the uterus, before the fluid can force its way out through the cervical canal by the side of the canula.

It is admitted, by all cautious practitioners, that injecting the uterus is often a somewhat hazardous operation, that may, when we see no reason to anticipate it, be followed by disagreeable consequences; and it is also admitted that to insure safety (if some kind of double canula be not used) it is indispensable that the cervical canal should be well open, so as to allow free regurgitation of the injected fluid. To insure this patulous condition of the cervical canal, it is a common practice to dilate it with sponge-tents. But the sponge-tent is unreliable, as the uterus is liable to contract soon after the removal of the tent. When, for example, the cavity of the womb is to be explored by the finger, we are directed to introduce a large sponge-tent, and to follow its removal, instantly, by the finger, *before the os uteri has time to contract.* If, *a fortiori*, we should defer the injection for a day or two after the removal of the tent, the danger would be much increased. We must therefore be put to the trouble (to insure safety) of introducing a tent every time we desire to inject the uterine cavity.

There is one important source of danger in uterine injections which I think has been too much overlooked, and which cannot be too much insisted on, viz., *obstruction to the return-current by coagula!*

Let us suppose, for example, that we are called upon to inject a uterus, somwhat abnormally dilated by sanguineous or mucous discharge: the organ may have a capacity of one, two, or several drachms. A double canula is introduced into the cavity, with its open mouths necessarily resting in the contained discharge. Suppose, then, a solution of persulphate of iron, tannin, nitrate of silver, chromic acid, etc., be thrown through one tube of the canula into the cavity of the organ, what happens? First, all the blood or mucus in the cavity is coagulated, and, if the injected fluid be forced on, it attempts to get out, both through the other tube and also through the cervix uteri beside the canula; but, these outlets being blocked up by coagula, the injected fluid cannot escape, and forcible dilatation of the organ results, if the fluid be still pressed forward.

In the above facts will be found the solution, I think, of the contradictory experience of gynecologists on the subject. When the object is simply to throw in ten or twenty drops of chromic acid, nitrate-of-silver solutions, pursulph. ferri, etc., this is easily accomplished by a proper syringe; but, where it is desired to wash the cavity out freely with water, simple or medicated, it is all-important that a free outlet should be secured.

The walls of the normal uterus are thick and strong; the openings to the Fallopian tubes not larger than a bristle; and, when we consider the contractile power of the organ, I am not inclined to believe that fluids (in the normal state) can be forced by injection through these minute orifices into the peritoneal cavity. Nor do I see why any *mild* fluid injected nto the uterus should do harm. Experiments on living animals have failed to make fluids, injected into the uterus, pass through these tubes into the abdomen, although the experiment sometimes succeeds on the dead subject, where contractility is lost.

It is true, however, that in certain physiological and pathological conditions a different state of things presents. For example, during and just after menstruation—during the existence of chronic uterine catarrh, in exceptional cases, in connection with polypi, and fibroids, and for some weeks after parturition or abortion, under all these circumstances the uterus is more or less enlarged, its tissues softened, and the Fallopian tubes not unfrequently more or less dilated. In such conditions, though we have little proof of the fact, we can well imagine that a fluid or air, thrown forcibly into the uterus, might occasionally find its way through these tubes into the peritoneal cavity. But let us approach, a little more directly, what I conceive to be the more probable cause of danger from uterine injections, viz., *the entrance of fluid or air into the circulation.*

I have already spoken of the peculiar structure of the lining membrane of the uterus; its great thickness; its close connection with the parenchyma of the organ; the mysterious connection of its blood-vessels with the utricular follicles; the curious and complicated net-work of vessels around each follicle; the frequent rupture of blood-vessels in this dense structure, which, like inodular tissue, is so well calculated to hold open their gaping mouths. We have also seen that in membranous dysmenorrhœa, and in certain inflammatory conditions, in pregnancy and abortions, the mucous lining, *the utricular follicles, and all, are thrown off entire;* that after labor this membrane is *not reformed under two months*, and not at all as long as inflammatory action continues. We have seen that the veins are remarkable for their size, and absence of an external coat, the internal being in immediate contact with the proper tissue of the uterus; that in certain diseased states the epithelium is cast off, the opposite surfaces presenting denuded patches, which come in contact, adhere and narrow the cavity either of the body or cervix at the internal os. We have alluded to the fact that granulations not unfrequently form, particularly on the posterior wall; and that these formations are much favored by retroflexion. And, lastly, we have seen that, in certain diseased conditions of the lining membrane, the net-work of capillaries is often so exposed as to be nearly bare; while in some instances these vessels may be observed hanging out loose in the uterine cavity, and giving the surface a villous appearance.

From the above *résumé*, it would seem that, in certain morbid conditions of the uterus (conditions, too, which there is reason to believe are not very rare), it is in the best possible state for admission into the circulation of air, or any fluid that may be forced upon its denuded vessels.

It has already been stated that the normal uterus, between the menstrual periods, has an internal capacity of not more than ten or twelve minims; but, while laboring under chronic catarrh and other conditions above alluded to, the organ may be found in a very different state. It becomes more or less enlarged, the walls sometimes thinned, the cavity dilated, the tissues softened, and in a condition to yield more readily to the distending force of a syringe. Even when the depth of the uterus, measured with the sound, is increased by disease an inch beyond its normal depth, its cavity is still a small one—say one to three drachms. The greatest dilatation usually occurs in cases of retroflexion, where the fundus is in a pendent position, and the cervix more or less mechanically obstructed. In such cases the body of the uterus is often dilated; the secretions are incarcerated and become putrid.

Suppose we determine to inject a medicated fluid into one of these relaxed, denuded uteri, with its utricular follicles gaping and its capillaries and veins exposed as we have described, according to Arthur Farre and other good authorities. The pipe of the syringe is introduced through the long, narrow cervix up to the fundus, the pipe pretty well filling up the cervical canal. The piston of the syringe is then forced down, and what follows? Sufficient fluid must be injected to fill the cavity of the body and distend it, before the cervix is sufficiently forced open to allow the fluid to regurgitate by the side of the pipe. Numerous instances have occurred under such circumstances, in which not only severe uterine colics have followed, but almost instant collapse, and even death in a short time.

How are we to explain these sudden and violent symptoms? Certainly, I think, not by the passage of the fluid through the Fallopian tubes, into the peritoneal cavity. The peritonæum has no such exquisite sensibility *until it has had time to inflame.* The abdominal cavity may be largely opened, as in ovariotomy; the peritoneal cavity brushed over with persulphate of iron, as I have seen both Dr. Sims and Dr. Emmet do; the pedicle of the ovarian tumor ligated or burnt with a hot iron, etc., without producing symptoms at all similar to those alluded to. There must, then, be some other explanation.

Is it not more reasonable to suppose that either air, or the injected fluid, is forced by the syringe *directly into the circulation?* When air or fluid is forced in among these denuded vessels, the organ distended, the relaxed utricular follicles pulled open, the veins and capillaries put on the stretch, what is to prevent the air or fluid in the syringe from entering any little rent or gaping mouth of a vessel held open, while the piston is pressed down?

Orfila has shown that a very small quantity of nitrate of silver, dissolved in water, and thrown into the vein of a dog, rapidly produces violent symptoms, followed by death. I need not tax your patience by repeating well-known facts and experiments illustrative of the fatal effects of air, salts, acids, etc., when injected into the circulation.

But suppose it be denied that nitrate of silver, and other remedies used in intra-uterine injections, can be thus *forced* into the circulation, upon what ground can the *absorbing* power of these exposed vessels be denied? Mucous membranes in their normal state have great absorbing power, but how much greater must it be when the epithelium is cast off, and the capillaries and veins exposed as I have described them! Prussic acid, either by a sound mucous membrane, or raw surface, is taken up so rapidly as to cause death in a minute or two. Morphia, atropia, strychnia, etc., when applied to a blistered surface, or thrown under the skin by a hypodermic syringe, will often be felt distinctly, in a minute or two. I have seen the bite of a rattlesnake kill a dog in two minutes.

Now, are not the denuded vessels of the uterus, as described by authors, in an exceedingly favorable condition for rapid absorption—quite as much so as any of the tissues above alluded to? It doubtless will be objected, that such articles as nitrate of silver, persulphate of iron, chromic acid, etc., which coagulate albumen immediately and constringe living tissues, are not likely to be absorbed. But the reasoning, though plausible, is not conclusive, and the comparative risk of these articles is an argument in favor of the idea of absorption of those articles of weak coagulating power. Nitrate of silver is often followed by violent symptoms—persulphate of iron rarely. Nitrate of silver *precipitates* albumen in minute flocculi, while the iron coagulates it into a gelatinous mass, which is unfavorable to absorption. I see no reason why a solution of nitrate of silver might not, under the pressure of a syringe, find its way into the interior of denuded or ruptured vessels, before any obstructing coagulum or constringing effect on the coats of the vessel has time to interfere. One fact seems certain, viz.: that a *solution* of nitrate of silver injected into the uterus is much more likely to be followed by sudden and violent effects than where the same salt is applied in solid form, or in that of an ointment—and so with many other articles.

Dr. Robert Barnes, after recommending the liquor ferri perchloridi fortior, half a pint to a quart of water, freely thrown into the uterus, after removing the clots, as a safe and reliable remedy for arresting *post-partum* uterine hæmorrhage, says: "I have also used it in cases of excessive flooding attending abortion; but where the uterus is small and the cervix not admitting more than a finger, I now prefer to apply the styptic on a swab, such as a common probang. I have a strong suspicion that, in one case of early abortion, an injection made forcibly by means of a caoutchouc bottle was the cause of a fatal catastrophe. The patient died almost suddenly, soon after the injection, with symptoms *resembling those consequent upon air entering the circulation.*"[1]

Courty tells us that Scanzoni reports a case of death, from injecting carbonic acid into the cavity of the neck, in one hour and three-quarters. He states, further, that it caused much excitement in Germany, and provoked some experiments on rabbits by MM. Breslau and Vogel, a *résumé* of which is given in the *Gazette Hebdomadaire* for 1858, p. 741; but Courty does not give the results of the experiments.

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[1] Half-Yearly Abstract, p. 235. 1870.

Nitrate of silver, taken into the stomach, is absorbed, carried into the circulation, passes off in the urine, and colors the skin. It forms an albuminate with the albumen of mucous secretions or of blood, and this albuminate is soluble in liquids containing alkaline chlorides, *which is the case with the secretion of the uterine cavity.*

The experiments of His and Ricklinghausen show that the tissues *imbibe* watery solutions of nitrate of silver, and it is by the aid of preparations made by imbibition that they have been able to *demonstrate* the existence of the epithelium of the lymphatics. Serous membranes imbibe it readily, and it passes to the subserous tissue.

Courty asserts, on his own, as well as the authority of Chomel, Aran, and Hardy, that acid nitrate of mercury, applied to the uterus, *salivates*—sometimes in a few hours, and for this reason he proscribes its use. Is it not easier to comprehend the absorption of a solution of nitrate of silver, than of a concentrated preparation of the acid nitrate of mercury, which instantly destroys the tissue to which it is applied?[1]

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[1] In the February and August Nos. of the *American Journal of Obstetrics* will be found two of the most instructive articles I know in print, on Uterine Injections, by Dr. Joseph Kammerer, of New York. From these articles I extract the facts given below.

"Astros, in Nélaton's wards, found fluid not in the peritonæum, but in a vein in the broad ligament of a female who died of facial erysipelas, after such injection."

"Copious injections (40 grammes) made with a urethral syringe—the force used being that ordinarily employed for injections into the ear—forced the fluid twice into the oviducts; in six cases the canal of the latter seemed to be impermeable; in one case the fluid did not pass through the tubes, but *entered the uterine blood-vessels;* in another of the cases ulceration of the inner surface of the uterus was found, by which the cavity communicated with the disturbed vein."—*Vidal de Cassis, Exp. on dead bodies.*

In his experiments on the dead subject, Klem says: "In three out of eighteen cases, blue ink, injected through a narrow os with moderate force, penetrated the venous system of the uterus and broad ligaments, without apparent laceration. To the latter circumstance is attributed the sudden appearance of metritis and peritonitis after injections."

"Bessems relates a case of sudden death after an injection of chlorine-

After all, however, it must be confessed that there are cases to be explained on some other principle than that of absorption. It happens in rare cases, that the mere introduction into the uterus of a small probe will be followed by colic, collapse, pelvic peritonitis, etc. Such cases can only be explained by the supposition of an extreme morbid sensibility in the organ, produced by a diseased condition we are unable to explain. Perhaps one of the best analogues we can give is that of urethral caruncle, so well described by Simpson. Not only do these extremely sensitive little growths occur in and about the urethra, but on the neighboring vagina. Sometimes a mere red speck—sometimes a growth as large as a millet-seed or pea, and I have seen one as large as a nutmeg. They are often, when touched, painful beyond description; and why may not the little growths so often seen within the uterus possess the same or even a greater sensibility?

There is still another class of cases, perhaps the most important of all, because the most frequent. I refer to those cases where the development is not immediate; but where a variety of secondary symptoms follow probing, cutting, and cauterizing—a day or two, or week or two afterward, such as pelvic peritonitis, cellulitis, salpingitis, ovaritis, metritis, etc. These cases are often slow in progress and obscure, but productive of great and protracted suffering. Owing to our imperfect means of exploring the cavity of the uterus, in the living subject, it must be confessed that the pathology of so-called *endometritis* is involved in much obscurity—that very different pathological conditions have been included under the same generic term; and that as a consequence our intra-uterine medication has been applied by most practitioners in a very empirical way. It has been too much the custom to thrust into the uterus the most irritating remedies without knowing the exact nature of the lesion—its exact seat, or whether the whole or part of the organ be diseased.

While it cannot be denied that this reckless practice is far too common, it is a pleasure to be able to bear witness to the honorable exceptions among the specialists of this city. We

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water. The *post mortem* revealed *air-bubbles in the vena cava* and the right ventricle of the heart."

have among us sober-minded, well-informed, observing men, who appreciate fully the dangers of reckless practice, and who never forget what the venerable Chomel so steadily taught, viz., that "the first and golden rule in medicine is, to be sure you do no harm."

To my mind, one of the most important principles in the local treatment of endometritis, and one which I have before insisted on, is the cleansing of the organ of its foul secretions, by frequent ablutions with a syringe. This is a plain surgical principle, that applies with as much force to the uterus as to any other hollow organ or diseased cavity. Mucus not only putrefies with great rapidity, but rapidly communicates the putrefactive process to blood. If putrid discharges be allowed to collect in the cavity of the uterus (as they are sure to do in flexions), it is difficult to imagine how recuperative action can take place; for not only do such discharges irritate the surface with which they are in contact, but doubtless more or less contaminate the blood. The insertion once in a week or ten days of a little nitrate of silver, chromic acid, Churchill's iodine, etc., cannot keep pace with the disease, while on the other hand experience proves that such remedies are not free from danger.

This principle of ablution, with some detergent wash, I think will be conceded, and the only question is as to its feasibility and safety.

It must be confessed that some degree of doubt, difficulty, and danger, should always attach to the use of uterine injections; that they should not be resorted to if we can gain our ends without them; and it is only the conviction that they cannot be dispensed with which has driven me to their employment. With proper caution, however, I think, the manipulation presents but little difficulty or risk. The uterus, as a rule, by gentle procedure, is soon educated to tolerate any medication we may think proper to adopt.

First, expose the os uteri with Sims's speculum, mine, or any other that allows the os to be caught with a tenaculum and drawn a little down so as to straighten the uterine channel and tighten its lining membrane; then introduce a double canula, with a very large eye like one I have devised, into the body of the organ. It has been objected that this canula is too large, but I reply that no uterus should be freely injected, into which an instrument of this size cannot be introduced—a free regurgitant current is the only guarantee of safety. If necessary, the os uteri must first be dilated with tents, dilating instruments of some kind, or the knife. When once open, the canula keeps it open.

As we cannot tell in each case, before testing it, what degree of sensibility the organ may possess, it is proper to proceed at the beginning with extreme caution. First, throw in through the canula, very gently, a little tepid water, or, what is better, a weak solution of morphia. If the uterus proves to be sensitive, continue this simple wash for several days, until the sensibility is overcome. Tepid salt and water is also very soothing. After toleration is established and a free outlet is secured, the organ may then be freely washed out. We may next move a little more boldly and substitute a weak solution of iodine, carbolic acid, or other drug thought advisable, and increase the strength if the case does not progress favorably. Bearing in mind, however, the important fact that, if we attempt to wash the uterus out by copious injections with articles that *coagulate albumen*, the return-current is arrested both *in* the double canula and at the internal os *around* it. Solutions of common salt, of muriatic acid, alum, or iodine, do not coagulate albumen. Also *weak* solutions of carbolic acid, permanganate of potassa, pyroligneous acid, and other similar antiseptics, may be used very diluted, for, although some of them do coagulate albumen, when dilute, they precipitate it in minute flocculi, that easily flow through any opening.

Muriatic acid not only dissolves the thick tenacious mucus of the cervix, but the hard blood-coagula formed by persulphate of iron. This fact I think worthy of note.

I will not say that I would proscribe the use *in toto* of solid nitrate of silver, or the concentrated solution of chromic acid. When other remedies fail me, I may be driven to their use, but not without fear and trembling, having fresh in my memory several cases in which deplorable results have followed the use of these remedies even in experienced hands.

Art. II.—*On Chronic Urethral Discharges.*[1] By F. N. Otis, M. D., Lecturer on Diseases of the Genito-Urinary Organs, in the College of Physicians and Surgeons, New York.

Mr. President and Gentlemen:

I propose for your consideration this evening the subject of *Chronic Urethral Discharges.*

In the term *chronic* it is intended to include, not only those purulent or muco-purulent discharges from the urethra which occur as the sequelæ of acute inflammations of the urethral mucous membrane, but all which, by their appearance and sub-acute character, resemble such discharges without regard to the time of their continuance. In consideration of the similarity between chronic urethral discharges, both as to their symptomatology and their chemical and physical constituents, and in view of the moral and social as well as the medico-legal elements associated with them, the importance of classifying such discharges in accordance with their etiology will, I trust, be admitted.

By such an arrangement, they readily separate into three distinct groups:

I.—The venereal specific.
II.—The venereal non-specific.
III.—The non-venereal.

Under the head of venereal specific we then have—

1. Gonorrhœa and its sequelæ.
2. Chancroid.
3. Syphilis.

Under that of venereal non-specific we have—

1. The menstrual fluid.
2. Vicious non-specific, vaginal, and uterine secretions.

Under that of non-venereal—

1. Acrid urinary secretions.
2. Idiopathic inflammation of the prostate.
3. Mechanical injuries and obstructions and chemical irritants.
4. Cutaneous disease.

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[1] Read before the New York Medical Journal Association, March 25, 1870.

The inclusion of gonorrhœa among the *venereal specific* causes of chronic discharge from the urethra is with entire acceptance of the fact that no physical distinction has yet been drawn between an acute urethritis, caused by contact with gonorrhœal matter, and one set up by the application of the purulent secretion of a conjunctivitis, or of a uterine catarrh, or by excessive coitus, or, in short, by any one of the causes which are set down among the non-specific class. Yet it is well known that a peculiar virulence does pertain to the purulent secretion of a gonorrhœal urethritis; that its contact with sound mucous membrane communicates with almost positive certainty an inflammation whose product is of similar virulence, and whose tendency is to run a prolonged course; while from all other causes to which a urethritis may be attributed not only is the establishment of the disease most exceptional, but when it does so occur is of shorter duration, and, as a rule, of more benign character. That gonorrhœal pus has the power, more than any other, of extending its degenerating influence beyond the immediate layer of epithelium in contact with it, also seems to me certain; but, until a specific virulent principle can be found in it, we must be content to accept the inflammation of gonorrhœa as a *simple* inflammation of unusually acute character. Yet, in a classification based upon etiological considerations, the propriety of placing gonorrhœa among the *specific* causes of urethral discharge must, I think, be conceded.

The organisms with which we have chiefly to deal, in considering diseases of the urethra, are mucous membrane, muscular and connective tissue, with their vessels and nerves. The mucous membrane which lines the urethral canal consists, like all other mucous tissues, of an epithelial structure, lying on a basement membrane; the epithelium being of the stratified kind, and of varied character in different regions—the tessellated variety presenting in the anterior, the spheroidal and columnar in the posterior parts of the canal. Underneath the mucous membrane is a thin layer comprised of muscular fibres and connective tissue, and is united to the tendinous layer of the corpus spongiosum by delicate membranous bands.

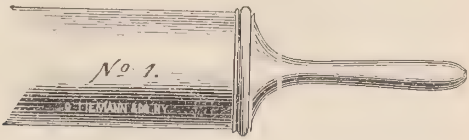
It may be well to glance briefly over the pathological changes which are now recognized as taking place in inflammations of mucous membrane of the variety under consideration, viz., those protected by a stratified epithelium. All such in their normal condition present a lubricated surface—this lubrication due to a bland secretion from the mucous follicles, poured out upon it.

Under the microscope, this secretion is found to consist of mucosine in which are suspended mucous corpuscles and epithelial scales. When the membrane is subjected to irritating influences, the epithelial element in the secretion is increased, the epithelial cells are hurried from the surface before they are fully developed, their forms become rounded, and hence they are more easily detached, until, as the inflammatory process becomes established, the natural proliferation of epithelial structure becomes luxuriation, and layer after layer is thrown off—less and less perfectly developed—losing more and more the characteristics of the true epithelial cell, until at length it has degenerated into the form recognized as the pus-corpuscle, and the mucous secretion has assumed all the features of the purulent discharge.

In their normal condition, mucous membranes secrete only sufficient fluid to answer the purposes of lubrication. All discharges from the urethra are then evidences of abnormal excitement—of imperfect cell-development—varying in degree from the first stage of epithelial imperfection to complete purulent degeneration, and dependent upon exciting causes of the varied character indicated in the classification I have ventured to adopt.

In the frequency of its occurrence, in the importance of its indications, in its pathological connections, and in its moral, social, and medico-legal belongings, the discharge arising from gonorrhœa and its sequelæ ranks first in importance. We have at this time only to deal with chronic forms of disease as defined in the commencement of this article. Omitting, then, all consideration of the acute stage of gonorrhœa, I shall at once proceed to consider the pathological conditions upon which a continuance of the chronic or subacute discharge may depend. These are as follows: 1. An enfeebled condition of that portion of the mucous lining of the urethra which has been occupied by the acute inflammation. The degeneration of epithelium set up by the acute disease is continued by *enervation*—a simple want of vitality in the tissue sufficient for a return to its normal functions. 2. The vitality apparently restored by appropriate local and general treatment; the discharge, though in decreased amount, still continues. Its continuance may depend on *the localization of the disease in the deeper parts of the urethra, or in folds of membrane, or in mucous crypts or follicles which have escaped local medication.* 3. The continuance of the discharge may be due to granular ulcerations located at any point along the canal where from any cause complete exfoliation of the epithelium has occurred. 4. From alterations in the *course* and *calibre* of the urethral tube dependent upon pathological changes occurring during recent or in previous inflammations. These causes of the persistence of a urethral discharge, with gonorrhœal antecedents, I propose now to consider, and to indicate the remedial measures which in my own experience have proved most productive of benefit.

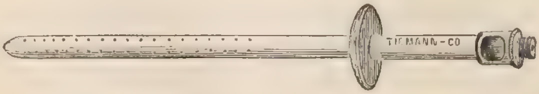
When, after a longer or shorter time, the acute symptoms of an attack of gonorrhœa have subsided, and there remains simply a purulent or muco-purulent, painless discharge, examination should be carefully instituted, with the view to ascertain the exact point to which the disease has extended, and, as nearly as possible, the pathological condition upon which the continuance of the discharge depends. This may be done in a rough way by pressing the walls of the urethra together and squeezing out the discharge from the meatus, making the pressure farther and farther back, until no more fluid can be made to exude. In the absence of any tenderness or uneasiness beyond the point so examined, you may conclude that the disease has not extended beyond that limit. If, in addition, you now carefully introduce a fair-sized bulbous bougie and fail with *it* in detecting any especial points of tenderness, you are at liberty to conclude that the difficulty is dependent upon the first of the causes mentioned, viz., *a want of recuperative power in the epithelial structure*, and that there is sufficient of the gonorrhœal influence to keep up an exaggerated desquamative action, though not sufficient to excite acute inflammation. The additional fact that the membrane is kept constantly bathed in fluid, also retards the return to a normal condition by diminishing the cohesive power of the superficial cellular growths. The indications for treatment then are, to apply such local means as are most likely to diminish the excess of fluid, and to stimulate the membrane to a more complete performance of its functions. Solutions of the salts of zinc, lead, and iron, combining the astringent and stimulating properties in various degrees, are found well calculated to meet this double requirement. Vegetable tonics and astringents are found also of value. The more thoroughly the epithelial products in the discharge are degenerated, the more stimulating and astringent is the application required to be; so that, when the discharge is thoroughly purulent, the more stimulant salts, as the chloride, sulphate, or acetate of zinc, etc., will be found most beneficial; the more it approaches the mucous character, the more simply astringent is the application required. Under all circumstances, where a simple atonic condition perpetuates the discharge, no solution of any sort should be used of a strength sufficient to produce a caustic effect. Stimulation alone is required, such as results from solutions—the sulphate of zinc, or the acetate of lead, alone or in combination, and of a strength varying from one to three grains to the ounce of distilled water. When the discharge is not wholly without pain, I am accustomed to add two or three grains of the extract of belladonna to the ounce. When the discharge is small in quantity and chiefly mucous, the acetate of lead, grains *one* to *three;* the persulphate of iron, grains three to five; tannic acid, from five to ten, are often promptly efficacious. The power of *phenol* (the so-called *carbolic acid)* to modify and arrest suppurative action, wherever located, is now generally admitted. My own experience in its use in disease of mucous membranes has been considerable, and I have seen positive benefit in quite a number of cases where a solution of two or three grains to the ounce has been used; but I have not employed it to any such extent as would at present warrant an expression of opinion as to its real value. The recent statement of a contributor to the *Cincinnati Medical Repertory*, that he had used it in *hundreds* of cases of gleet with *magical* effect, suggests a prevalence of the disease in that region which is appalling; while an entire forgetfulness to cite the supposed pathological conditions in any case, would warrant a suspension of judgment as to the accuracy of the recital. Other journals have presented testimony of its efficiency in the treatment of urethral discharges. The antiseptic and antiparasitic qualities of *phenol* certainly warrant an expectation of usefulness in discharges of a specific nature, and it seems to me not improbable that it may come to be a valuable agent in the management of gonorrhœal disease. The permanganate of potash, three to five grains to the ounce, has been highly recommended in simple chronic gonorrhœa. I have used it in perhaps twenty cases, with the apparent effect of arresting the discharge for a short time, but have invariably been obliged to resort to other means to complete the cure. The mode of application of solutions to the urethral mucous membrane which I have been accustomed to employ is by injection, with a hard rubber syringe, of the capacity of half an ounce, and constructed with a well-rounded extremity, so that it may be easily and painlessly introduced, and that the meatus may be readily and effectually closed around the pipe after insertion. Inasmuch as it is desirable that the injection should be applied only to the diseased surface, the urethra should be closed by pressure with the thumb and finger at the point which you have previously fixed upon as the depth to which the disease has penetrated; with this precaution you will also avoid the danger (which is not an imaginary one) of establishing a new focus of disease by forcing the vitiated secretions into the deeper parts of the urethra, or even into the bladder. A very general impression exists among our profession that fluids are with difficulty injected into the deeper parts of the urethra



by an ordinary syringe, and that to force them into the bladder, by that means, is a physical impossibility. The imperfect construction of syringes in general may in part account for such an impression, and the positive statements to that effect by various authors (Acton, Miltön, etc.) would tend to confirm such a belief. Within the past two years I have had three patients who were able to inject their own bladders by means of an ordinary Davidson's syringe, one of them throwing in a pint of water in my presence, then emptying the viscus—refilling and discharging it three times in succession. I am therefore convinced that it is judicious to limit the distance we desire to medicate by pressure on the canal at a given point. And I also believe that the whole diseased surface can usually be reached by a properly-constructed syringe of ordinary size. After directing the patient to pass his water (for the purpose of cleansing the canal), the medicated fluid should be thrown in quickly, to avoid spasmodic resistance, filling the urethra to the desired limit, and allowing it to remain for from one to three or four minutes. This procedure I am accustomed to have repeated three or four times in the twenty-four hours.

If, notwithstanding the use of injections, administered after the manner I have indicated, the discharge still continues, though in decreased quantity, no other cause of failure appearing prominent, I am led to infer—

2. That the medicating fluid does not reach all points of the diseased surface; that, from insufficient distention of the canal, portions between folds of the membrane, or in the sulci of some of the numerous follicles with which the urethral lining is studded, have escaped the topical application. For security against failure from this cause, I am accustomed to introduce the injection through the accompanying modification of



the ordinary syringe. By means of this instrument the urethra is penetrated to the farthermost point of disease—distended to its full capacity and thoroughly bathed with the contained fluid. No point or portion can escape the application, except it be located in the lacuna magna, or in some accidental follicular sinus. These exceptions I am inclined to think are not very rare. Dr. Benjamin Phillips, in his treatise on "Diseases of the Urethra," states that he has found the continuance of a chronic gonorrhœa to depend upon the engagement of the *lacuna magna* in the disease, and cites four cases of cure by slitting up the inferior wall of that sulcus on a director. I have met with two cases of similar character which were successfully treated by injections introduced with the aid of a blunted hypodermic syringe. Under the designation of "*follicular sinuses*," I allude to little fistulous canals which are sometimes met with running outward from the urethra, and occasionally opening upon the surface of the penis.

I have a record of two and possibly of three such instances. The first, in a gentleman who presented himself to me suffering from a very scanty muco-purulent discharge of two years' standing, which, commencing as a gonorrhœa, had resisted much treatment. Close to the meatus—say a quarter of an inch—on the right side, two minute openings were visible, each the size of a pin's-head, one above the other, and about one-fourth of an inch apart. The patient remarked that, after connection, he always noticed a little matter at these points. Examining the fossa navicularis, I found its floor occupied by a narrow superficial ulcer a third of an inch from the orifice and half an inch in length. Exploring the fistulous openings with a fine probe, I endeavored to find a communication between them and the ulcer of the fossa, but was unable to do so. I did find, however, a fine canal connecting the two abnormal orifices, which I slit up and cauterized. The ample meatus received a No. 20 (French) bulbous sound with ease, but was arrested at the point of ulceration, and would only allow the passage of No. 16. I slit the constriction, which extended the entire length of the ulcer, and passed a No. 20 Beineke sound into the bladder without difficulty. Twenty days after, the wound was cicatrized, and the discharge had disappeared. I felt confident to the last that there had been a connection between the openings on the surface and the ulcer of the fossa, but failed in establishing it. Five years have passed since then, but the patient, who married about that time, has had no further urethral trouble.

The second case was that of young man from Omaha, who came to me presenting a pustule the size of a pin's-head on the right side of the meatus urinarius, midway of the glans, and about one-third of an inch from the labium. Believing it to be the result of a vicious connection four days previous (as it had quite the appearance of a follicular chancroid), I cauterized it with a fine glass point charged with nitric acid, and felt warranted in giving the assurance of speedy cure. Two days following, the patient presented himself, with the lesion cicatrized, but a similar pustule had developed about a quarter of an inch above the site of the first. Confirmed by this, in my view of the chancroidal origin of the difficulty, the second was likewise touched with the nitric acid. On the following day my patient again presented himself, announcing that the first pimple had again broken out, and that he also had the *clap*. Making pressure of the glans, a drop of creamy pus exuded from the meatus and also a minute quantity of the same sort from the two little orifices on the site of the pustules. Struck with the similarity in location and appearance of these little openings with those of Case I., I at once set about exploring them. A fine silver-wire probe passed readily into one and out at the other; the lower seemed superficial. Into the upper, however, I succeeded in passing the probe nearly half an inch backward and upward on a plane parallel with the urethra. Feeling certain that a communication existed, through this sinus, with the urethra, I introduced as far as I was able the blunted point of a fine hypodermic syringe; and, having previously insinuated a bit of lint into the fossa navicularis, I injected a solution of indigo. After several unsuccessful trials, at last, on the withdrawal of the lint, it was found slightly but distinctly stained with the indigo. Shall we infer in this case that the trouble was originally a simple folliculitis creeping along an accidental sinus--possibly producing it--opening on the surface of the glans, and finally breaking also into the fossa? or, was it of gonorrhœal origin, contracted in the external follicular opening, and after seven or eight days cropping out into the urethra?

No solution of continuity could be detected in the fossa navicularis, nor was there much tenderness present at any point. A ten-grain solution of the nitrate of silver was injected into the fistula, with the apparent effect of closing it up entirely: the passage between the two points was slit up and cauterized. The gonorrhœa (if it was a gonorrhœa) extended very little beyond the fossa of the urethra, ran a very mild course, and ceased under astringent injections in about ten days.

The third case was in a Mr. D., who came to me two years since, complaining of a little boil on his penis. Examination disclosed a small purulent-looking collection between the folds of loose tissue a little to the right of and behind the frenum. Both the surrounding inflammation and the swelling were very slight; there was but little accompanying tenderness; the deposit was covered only by the transparent cutis. A slight touch with the bistoury caused it to discharge three or four drops of laudable pus. As there were no venereal antecedents connected with the case, I remarked that it was probably a little sebaceous follicle which had become obstructed, and that he would have no further trouble from it. Several weeks after, Mr. D. called to inform me that he was quite well of the boil, but that, when he urinated, the water came out of the side of his penis. On examination, I discovered a fine opening like a pin-hole at the bottom of a small funnel-shaped depression on the site of the old difficulty. A fine silver-wire probe readily penetrated it, parallel with the urethral canal, for about half an inch. Failing to find my way into the urethra by this means, I introduced the blunted hypodermic syringe, and, on driving in the piston, the existence of the fistulous communication was demonstrated by a free dripping of water from the meatus.

The foregoing cases, taken together, appear to me to warrant the inclusion of follicular sinuses among the possible causes of persistent urethral discharge; and, although I find no mention made of such complications in the literature of urethral disease, I venture the opinion that analogous cases have occurred in the experience of many practitioners. In conjunction with the local treatment, an internal administration of such special medicines as are known by experience to act beneficially upon diseased mucous tissues, especially those of the urinary tract, is often advisable.

The balsam of copaiba and the oil and powder of cubebs I have prescribed with benefit, but so often have succeeded in upsetting the digestive apparatus of my patients, without securing the desired result, that I now rarely recommend their use. Much more tolerable, and in my experience of much greater efficacy in such conditions, is the *oleum santalum citrinum* (the oil of the yellow sandal-wood), administered in doses of from ten to twenty drops on sugar, or, preferably, in capsules, three or four times a day. I have seen recoveries from its use in from three to six days after the long and faithful use of injections and other internal medicines had proved unavailing.

Berkeley Hill, a recent English writer, speaks highly of this remedy, where it can be borne, and advises its use in doses of from twenty to sixty drops, three times a day, remarking, however, that "it produces nausea and vomiting, like copaiba, though in less degree." The maximum dose I have ventured to prescribe has been twenty drops; and, when in uncomplicated atonia of the urethral membrane, always with good effect. A patient would now and then complain that the subject of sandal-wood fans was too often introduced in his presence to be quite agreeable; beyond the *odor*, however, and an occasional slight dyspeptic trouble, the remedy appeared unexceptionable. Recent chemical investigations[1] have demonstrated the presence of *phenol* in the aromatic oils, such as oil of thyme, fir, cinnamon, cubebs, sassafras, sandal-wood, etc., and have included them in the class of *phenols*. Not a few of these, cubebs, fir, thyme, etc., have long had a reputation for usefulness in diseases of mucous membranes generally, and in gonorrhœal disease in particular. Is it not possible, then, that the active curative property in each is due to the *phenol* it contains, and that its predominance in any one determines its superiority? Further chemical researches in this direction may yet furnish other and still more potent remedial agents for internal as well as local use, and aid in the simplification of our now too empirical and overburdened category of anti-blenorrhagic remedies. The muriated tincture of iron, in doses of from ten to twenty drops, repeated three or four times a day, I have found of decided benefit in asthenic gonorrhœa, even when the patient did not present the usual constitutional aspect which indicates its use. Iron, in conjunction with cantharides (as recommended by Dr. Bumstead, page 90 of his "Treatise on Venereal Diseases"), I have occasionally prescribed, with prompt beneficial results. In cases of long standing the discharge is often found to proceed chiefly if not wholly from the deeper parts of the urethra, the bulbous, and even the prostatic portion. Treatment in such cases need be in no wise different from that already indicated, excepting perhaps in the use of long pipe syringes, to secure with certainty application of the local remedial agents to the entire diseased surface. Where the bladder has evidently participated in the difficulty, as announced by uneasiness and aching in the supra-pubic region, with or without increased irritability of the vesical sphincter, and confirmed by the presence of pus in the urine drawn directly from the bladder, a daily washing out of this viscus with a solution of Squibbs's perchloride of iron, twenty or thirty drops to the pint of tepid water, has usually, in my hands, proved promptly successful in relieving this complication.

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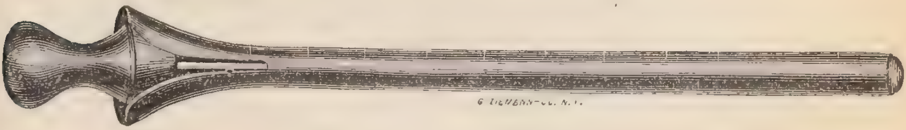
[1] See Squibbs's "Notes on the Phenols from Coal Tar," etc., in the proceedings of the American Pharmaceutical Association, 1868.

In cases where the discharge persists, notwithstanding a faithful pursuance of the above plan of treatment, and no constitutional complications are recognized, I am led to suspect the existence of the third in the list of causes upon which a continuance of the discharge may depend, viz.: a granular condition at some point or points in the canal, where, from an unusual activity of the morbid processes, the mucous membrane has been completely stripped of its epithelial covering: the underlying tissue participating in the inflammatory process, ulceration results. At a certain stage in the declining inflammation, little irregular papillæ organize and sprout from the plastic lymph which has been exuded to repair loss of tissue, and these papillæ we call granulations. The tendency of this loose cell-growth to condense into fibrocellular tissue renders the granular condition one of great importance, on account of its ultimate tendency to produce contractions, and hence narrowings, of the urethral canal. It is likewise frequently a source of free purulent secretion.

The granular condition is usually indicated by a localized tenderness on pressure, or on the passage through the urethra of a sound or bulbous bougie. These methods of ascertaining the presence of such a complication are, however, liable to lead to incorrect conclusions, inasmuch as such tenderness may be produced by the natural obstructions to the introduction of an instrument, as at the triangular ligament, the commencement of the membranous portion, also at the neck of the bladder. Besides this, such granulations may exist, and yet, on account of the absence of nerve-structure in them, produce no decided sensation on the passage of instruments. In this dilemma we have fortunately another resource, viz.: *the ocular inspection of the entire surface of the canal* by means of tubes of proper construction, which may be introduced into the urethra and illuminated, so that every important point is made visible to the careful observer.

As far as known, the credit of first applying ocular inspection to the diagnosis of urethral difficulties is due to Dr. J. D. Fisher, of Boston, who, in 1824, published, in the *Philadelphia Journal of Medical Science*, the description of an instrument identical in all essential points with the endoscope of Desormeaux. At present, however, the names of Desormeaux, of Paris, and Cruise, of Dublin (who improved upon the illuminating apparatus of Desormeaux's instrument), are alone associated with the endoscope.

By their patient and careful observations and experiments, and by their large and valuable contributions to the pathology and treatment of urethral diseases through its use, they are entitled to stand eminent as authorities in that especial province. The endoscopic tubes of Desormeaux and Cruise were constructed of white metal, eight or nine inches in length. In using them I found a serious objection to the metallic surface, on account of the troublesome play of reflection along their interior; and, moreover, so great a length appeared unnecessary for examinations of the anterior portions of the canal. Tubes of various lengths, varying from one and a half to eight inches, were constructed of hard rubber by Messrs. Tiemann & Co., under my direction. The even, black surface of these tubes, though requiring somewhat stronger light, was entirely free from reflections, and enabled me to define with much greater certainty appearances in the field or bottom of the tube. These were distinct under reflected sunlight, and also from that thrown out by Tiemann's modified student's lamp, burning kerosene-oil, with the addition of ten grains of gum-camphor to the ounce. The use of tubes of various lengths made it possible to bring the eye much nearer the desired surface, when it was located at any point anterior to the prostatic urethra, and proved, also, of further advantage in the greater ease with which the light was kept steadily on the field of the tube, and likewise, the shorter tubes perceptibly decreased the absorption of the luminous rays. Like Desormeaux's tubes, they were furnished with an entering shaft, to facilitate introduction into the urethra, and a mortice or cleft in the side, for the greater facility of making applications through them. For accuracy in locating any seat of trouble, they were graduated in one-half inches, and, to distinguish them, were called meatoscopes.



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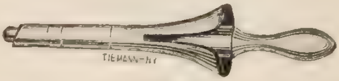
I have used these instruments exclusively for the five years past, and believe I am able, through their assistance, to detect the more important pathological changes occurring in the urethral interior. Especially is the meatoscope valuable in diagnosis of the granular condition of the urethra, previously mentioned. Introduced beyond the suspected point (the shaft being removed), a pencil of light is reflected, by means of a small concave mirror into and to the bottom of the tube, which is then slowly withdrawn. The folds of healthy membrane, rolling symmetrically in toward the centre, enable the observer to note the exact point of departure from a healthy condition, and the character and extent of the lesions. The favorite seats of granular ulceration of the urethra are in the natural expansions of the canal at the navicular and bulbous portions, evidently invited by the rich diffusion of crypts and follicles in the ample folds of those parts. Not seldom the difficulty, when occurring in the fossa navicularis, is occasioned by frequent and teasing contact with the point of the urethral syringe. No especial localizing cause being present, we should expect to find granular ulcerations of more frequent occurrence in the deeper portions of the urethra, on account of the preponderance there of the spheroidal and columnar varieties of epithelium, the cells of which, less easily detached, are also less readily reproduced than those of the tessellated kind. The location and nature of the diseased surface being determined, topical applications may be made through the reëntered tube, by means of wire stylets armed with a bit of cotton or lint soaked in the fluid selected for use.



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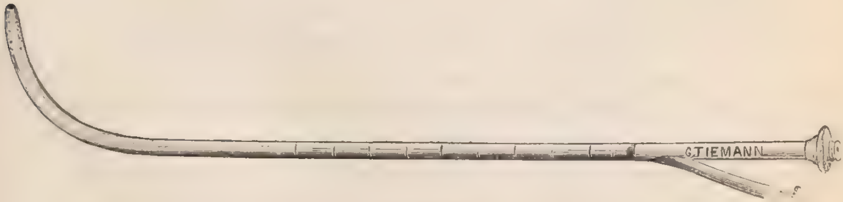
I am accustomed to use for this purpose a solution of the nitrate of silver, of a strength graduated in accordance with the sensitiveness of the parts, of from *twenty* to *thirty* grains to the ounce of distilled water, usually limiting the application to such an extent of surface as can be touched with the cotton at one time—the tube being held stationary. I have not unfrequently extended the application from one quarter to one-third of an inch, sopping the surface as the tube is slowly withdrawn. I have not usually found it necessary to inspect the urethra under the light after having thoroughly located the diseased surface. Observing carefully the mark on the tube when the first application is made, it will usually be found that this portion is quite free from pain on the succeeding introduction of the tube, and that commencing sensitiveness indicates the point to which cicatrization has extended. At the first sensitive spot I repeat the application as before—advancing gradually, at each sitting, until the entire lesion is restored. Applications may be repeated once in from four to eight days. The field of the meatoscope should be carefully cleansed from any discharge that may be present, both in the preliminary examination and also in that immediately preceding the treatment—this is easily effected by means of a bit of cotton twisted on one of the stylets. In some cases of granulated urethra, I have used, instead of the foregoing plan, an injection of four or five drops of a solution of similar strength, with good results. For applications to the prostatic region—less easily reached with the straight tube of the meatoscope—the use of a long curved syringe has appeared to me preferable. When pursuing this plan, for a long time I was in the habit of locating the prostatic region by the preliminary introduction of a catheter into the bladder, measuring back half an inch from the point where the water entered the catheter as about the central part of the prostatic urethra, then transferring the measurement to an ordinary long curved syringe. I was thus enabled to apply injections with accuracy to the desired point. About a year since I had occasion to see a patient with Dr. James Bigelow, of Brooklyn, and apply an injection to the prostatic urethra, after the manner above described. The entire success of a single application in relieving the diseased condition was so gratifying to the doctor, and the bungling means through which it was accomplished so apparent, that he designed and presented to me soon after the accompanying syringe-catheter. By means of this



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ingenious instrument, both the measurement and the application are accomplished by a single introduction. The shaft of the instrument (which I now exhibit to you) encloses a double canal—one continuous with the barrel of the syringe, following the inner curve of the instrument, and terminating at the curved extremity of the shaft in a number of minute openings, the other canal continuous with the little branch-tube, and following the outer curve to the extreme end of the shaft. This (the catheter portion) is traversed by a wire which stops the opening at its extremity. On the introduction of this instrument, with the wire slightly retracted, at the instant of its entrance into the bladder a few drops of urine exude from the branch tube; the wire-stopper is then pushed in, the instrument withdrawn half an inch, and the piston driven home. For all topical applications to the prostatic urethra, this instrument has, in my experience, proved admirably adapted.

The last, but by no means the least important in the list of local causes which are capable of prolonging a chronic gonorrhœal discharge, is the alteration which may occur in the course and calibre of the urethral canal. Henry Dick, of London, who has written an elaborate monograph on the pathology of gleet, asserts that the continuance of a gonorrhœal discharge may depend on *deviations in the course of the urethra without contraction of its dimensions.* His diagnosis in such cases was arrived at, by noticing, in cases where no actual disease could be detected and the discharge continued, that, on introduction of a sound, the flat handle always became oblique in the membranous portion of the canal—that wax bougies used for diagnostic purposes, when withdrawn, were crooked at that point, while no evidence of constriction was noticeable, and that these cases were cured by the systematic introduction of sounds.

I have seen cases which apparently presented all the above-named peculiarities, but was, and am still, of the impression that the irregular muscular action of that portion of the urethra produced the seeming deviation, and that the continuance of the discharge was dependent upon a lack of suppleness from general but superficial contraction or fulling up of the mucous membrane, which disappears, and along with it the discharge, on the return of the urethra to its normal dimensions.

Most frequently, however, chronic gonorrhœal discharges depend for their continuance upon positive and recognizable alterations in the calibre of the urethra—contractions at various points of the canal—the legitimate sequelæ of follicular ulcerations.

As the urine is propelled through the urethral tube, it impinges with more or less force upon any salient or contracted point. The column of fluid is arrested; in proportion to the degree of arrest is the force of the blow upon the mucous surface at that point. More or less hyperæmia necessarily ensues, and a condition is soon established well adapted to prolong an existing gonorrhœa, or which, upon slight additional cause, such as venereal excitement, or even an unusually acrid condition of the urine, may result in the establishment of a muco-purulent or a purulent secretion. We may then affirm, as a most important axiom, *that the slightest encroachment upon the calibre of the urethral canal is sufficient to perpetuate a urethral discharge, or even, under favoring conditions, to establish it, de novo, without venereal contact.*

It is in this way that gonorrhœas occurring a few hours after exposure are *generated;* and it also explains the apparently mysterious breaking out of a urethral discharge after excitement, in individuals who have had no gonorrhœal disease for years.

Within the last two months, a gentleman, who (according to his own account) had lived virtuously for more than thirteen years, consulted me in regard to a muco-purulent discharge which gave him painful suspicion of the fidelity of his wife. I found, on examination, that it was dependent upon a stricture at the commencement of the membranous portion of the canal which scarcely admitted a No. 3 bougie, and yet no suspicion of stricture had before arisen.

Some four years since, a young man came to me in great distress, requesting an opinion as to the probabilities of contagion from a muco-purulent discharge from which he was then suffering. He had a history of an acute attack of gonorrhœa a year previous, which was cured, all but an occasional very slight oozing of yellowish matter. This, after ten months' persistence, was pronounced innocuous by his medical adviser, whereupon, he went to Chicago and married. Three or four days after, finding his discharge increase, he left his bride and came to this city, to inquire concerning the possibilities aforesaid. Examination in this case brought to light a narrow stricture at the peno-scrotal angle, which had evidently perpetuated the discharge. In regard to the contagious property of such a discharge, I will simply state that, within the week he read me a letter from his bride, containing as classical a description of gonorrhœa in the female as I ever saw.

It will be readily seen that the recognition of stricture as a cause of the origin or the persistence of a muco-purulent discharge is of the utmost importance, involving, besides the discomfort of the local trouble, other issues of the gravest moment.

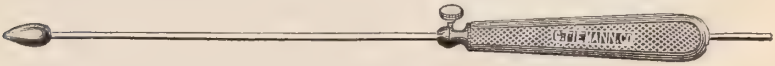
While authorities differ as to the precise seat in which contractions may occur, all are agreed that their most frequent locations are those points where gonorrhœal inflammation runs the highest and dwells the longest, viz., at the bulbous or navicular parts of the urethra.

The chief if not the sole cause of these contractions is the granular ulceration previously dwelt upon. The plastic lymph then thrown out, becoming organized and finally condensing or cicatrizing, produces constriction of the tube to a greater or less degree, in proportion to the extent of tissue involved—forming, in short, what we are accustomed to designate as *stricture of the urethra.*

I have already spoken of the difficulty of diagnosticating contraction by means of the ordinary sound or the flexible bougie. The simple fact that a No. 16 or even a No. 20 can be readily introduced, is no positive proof of its absence. A man may have a urethra of a size No. 21—there may exist a contraction at some point, in the course of the canal, of half a line in depth, and yet the instrument No. 20 will fail to announce its presence. To obviate this source of error, Sir Charles Bell, many years since, invented the *ball-probe,* which consisted, as the name implies, of a slender rod surmounted by a metallic ball.

Selecting one suited to the proportions of a given meatus, it was passed down the urethral canal until arrested by the stricture; then this, or one of a size just permitting its passage through the contraction, was introduced through it and allowed to remain for a few moments. On attempting to withdraw the instrument, the ball would be arrested at the posterior boundary of the stricture—thus definitely locating its extent and position.

Le Roy d'Etiolles improved the ball probe of Bell by substituting an acorn-shape for the ball and a flexible material for the shaft, thus facilitating its introduction, and adapting the shaft more readily to the curvatures of the canal. In my own practice I have given a preference to still another modification which appears to me to combine the excellences of both instruments, viz., a metallic *olive-shaped* bulb, whose firm polished surface (like Bell's) glides more readily over the mucous membrane than the gum coat of d'Etiolles's bulb—the olivary shape, while entering as easily and defining with sufficient accuracy, is less painful on withdrawal than the more abrupt base of the acorn-shape, and the small soft metal shaft which unites greater firmness with a degree of flexibility sufficient for ready alteration of its curve. The handle is perforated throughout its extent, thus allowing it to be slipped forward on the shaft to the meatus, and screwed fast, so that, when the situation of the stricture has been determined, it correctly registers the depth of the contraction. In explorations of the urethra with this instrument, I am accustomed to accept the meatus, if apparently of normal size, as a gauge of the urethral calibre; that is to say, any instrument which will pass that orifice will easily traverse the entire canal if no abnormal condition be present. It should be borne in mind, however, that both congenital and pathological contractions of the meatus are not infrequent.



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A bulb, with its shaft bent to correspond with the curve of an ordinary sound, is accurately fitted to the urethral orifice, then slowly inserted and pushed gently back until some resistance is recognized. Muscular contraction may arrest the instrument at any point along the spongy urethra; but, with a little delay, this will subside. As the bulb advances it may impinge upon the triangular ligament; tilting the shaft upward will clear this point. Muscular contraction will also usually occur at the commencement of the membranous portion and at the posterior part of the prostatic; gently overcoming which, the bulb slips into the bladder. This is the usual course of the proceeding when no contraction has been recognized. Now allowing the bulb to remain in the bladder for three or four minutes, it is slowly withdrawn; if contractions exceeding half a line in thickness be present at any point, slight clinging or want of suppleness will indicate their locality, and, in drawing the bulb back and forth where resistance is appreciated, a diagnostic ridgy feel may be recognized. Should this proceeding fail in locating a constriction, I am then accustomed to slit up the meatus freely and repeat the operation with the largest bulb that will enter the spongy portion. Failing with this, a full-sized meatoscope, without the entering shaft, is introduced under the light and slowly pressed back along the passage, carefully noting if any paling, or lack of flexibility of the membrane, occur at any point. Should this last effort yield no evidence of undue condensation of tissue, I am forced to conclude that no contraction is present.

Decided stricture is not likely to escape notice; it is the *slight* diminutions of the urethral calibre that are usually overlooked, and which may keep up, indefinitely, a troublesome discharge. Especially at or near the meatus is stricture likely to elude observation, and, in my opinion, the occurrence of stricture at these points has been greatly understated by authorities. Not only is the inflammation unusually acute at this extremity of the canal, but the irritation, and even excoriation, which often results from the use of improperly-constructed syringes, plainly increases the tendency to plastic effusions about the urethral orifice. I will cite a single instance in point. A gentleman consulted me not long since on account of a muco-purulent discharge from the urethra, stating that he had never had gonorrhœa, nor any suspicious exposure for many years. I found a decided contraction at about a third of an inch from the meatus. On inquiry I ascertained that, in times past, when he was in the way of carnal communication with loose women, he was in the habit of using an injection of alum-water as a preventive of disease. The stricture appeared in this case—which I consider typical of a class—to be due to the teasing contact of the membrane with the point of the syringe. The presence of a warty or polypoid growth may interfere with the integrity of the urethral calibre—a complication which can be readily recognized by means of the meatoscope.

The treatment of contractions of the urethral canal cannot be fully considered in the limited scope of this paper. I am in the habit of employing the usual methods—gradual dilatation, rupture, or internal division, according to the indications presented in each case. Always bearing in mind the tendency of strictures to recontract, I endeavor to leave the strictured part *above*, rather than equal, its normal size; using healthy portions of the given urethra as the guide for that size, and *not* the numbers marked on the sounds. The standard measurements of the normal urethra are very well to philosophize upon, but, practically, we must recognize and respect a distinct individuality in each, irrespective of standards, or even of general physical proportions. A few days since I introduced with ease a No. 20 English sound into the bladder of a boy of sixteen, not overgrown. I have frequently seen adults whose normal calibre did not exceed half that size. The not uncommon remark that such-and-such a sized sound has been passed, and consequently no stricture can exist, leads to frequent error.

Engorgements of the urethral tissues occur readily after the discharge has nearly or entirely ceased. The introduction of a sound, or even the passage of urine, may cause the lips of the orifice to become suddenly florid, as though acute inflammation were present—evidently exhibiting a want of contractility in the vessels of the part. Patients likewise complain of an aching sensation along the urethra, and especially the perineal portion, where the deeper parts of the canal have been involved in the preëxisting inflammation. For this condition I have been accustomed to make applications to the relaxed membrane of equal parts of the submuriate of mercury and tannic acid, by dipping an undersized bulbous bougie in oil, then rolling it in the dry powder, previously mixed, introducing the instrument with care until the bulb enters the bladder, then slowly withdrawing it with a twisting motion.

Sounds with little cup-shaped depressions at their extremity (designed Dr. William H. Van Buren, of this city) are also valuable for carrying medicated unguents into the urethra. The judicious introduction of *unmedicated* sounds or bougies of large size is also beneficial.

For the same purpose I have frequently applied free carbonic-acid gas to the urethra, throughout its entire extent, by means of a flexible catheter attached to an indian-rubber gas-receiver of two or three gallons' capacity. The receiver is placed in a chair opposite the patient. Passing the catheter down to the prostatic urethra, the stop-cock is turned and pressure made upon the receiver by the knee of the operator. The escape of the gas along the sides of the catheter, with a sputtering sound, announces the successful application of the gas to the entire mucous surface of the canal. The tonic and sedative effect of this procedure is prompt and curative in many cases of abnormal sensitiveness of the urethra, following chronic inflammation.

[TO BE CONTINUED.]

ART. III.—*Remarks on the Reproduction of Bone.*[1] By WILLIAM R. WHITEHEAD, M. D., New York.

THE conditions of health and of disease which influence the development of bone are numerous and diversified, and, like those of most vital phenomena, are exceedingly difficult of accurate appreciation. But these conditions, we may safely assume, are quite as constant as those which govern the development of the less complicated phenomena of inorganic matter.

Although the physiological processus of osteogenesis has been studied as carefully as De Lisle first studied the laws of the formation of crystals, yet the complexity of the vital conditions of cell-development would seem to preclude the application of as precise an analytical reasoning to cell-formation.

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[1] This paper was read at a meeting of the New York County Medical Society, March 21, 1870.

Haüy, who first applied geometry to mineralogy, is said by Buckle to have laid the foundation of the pathology of the inorganic world.[1]

This distinguished historian[2] further remarks, in his searching generalizations, that symmetry is to crystals what health is to animals: in fact, pathological phenomena are but deviations from the regular workings of natural laws of symmetry or health. There are minor laws which disturb the greater or general laws; these first, whether considered separately as influencing the perverted action of cell formation in bone, or of perverted nutrition in other parts, produce an irregularity which is more or less regular and constant in its action. This irregularity, as seen either in the abortive crystallization of a mineral, or in the arrested development of an animal or vegetable cell, is regular and subordinated to a definite law, which, if we succeed in discovering it, will be characterized by as great a uniformity of sequence as the most important law of Nature.

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[1] Since this paper was read there has appeared an interesting article in the *Journal d'Anatomie et de Physiologie*,[1] concerning experimental researches on changes in the composition of bone. The author has prefaced the recital of a few experiments with somewhat lengthy remarks about the employment of a mathematical method for the accurate determination of biological subjects of study. It is evident that the teachings of Comte, in his "Philosophie Positive," have commenced to exert a gradual and more extended influence on the study of subjects which heretofore have not been generally subjected to exact methods of thought.

The present age is eminently distinguished for the accumulation of facts; the time approaches for the correct coördination of these facts, especially for the data furnished by physiological experimentation. The physical sciences are rapidly supplying the means of a more accurate registration of these data; or, in other words, the terms of certain biological equations are being defined with more precision.

The tension of the arteries and the force of the heart, as enregistered by the tracings of the sphygmograph, afford an example of the application of a delicate mathematical instrument to the determination of physiological problems.

[2] Buckle's History of Civilization in England.

[1] Recherches expérimentales sur les modifications de la composition immédiate des os, par M. Fernand Papillon, March and April No. of 1870.

But an accurate method, combined with careful observation and experimentation, is indispensable for the successful study of most vital phenomena; especially those of the reproduction of bone in general, in order to understand the remarkable peculiarities which sometimes attend its development—peculiarities which are due to changed conditions, and the adaptation of different tissues to those conditions.

During the eighteenth century was originated and developed in France the most important of the physical sciences, and, as one of the minor consequences of the extensive application of the spirit of research, an important discovery concerning the growth of bone by the periosteum was made by one who was not a physician, but who was distinguished for the universality of his acquirements. This person was Duhamel. His researches in vegetable physiology distinguished him as a botanist; his versatile genius and spirit of generalization prompted him to connect similar phenomena of animal and vegetable physiology; and his intelligent spirit of inquiry urged him to subordinate analogy to experimentation. The property of the periosteum to produce bone was suggested by the growth of a tree from the hardening of its cortical layer, and was confirmed by experiments with madder on animals—experiments which about a century later were supplemented by Flourens, who proved that the size of the medullary canal was increased by absorption of osseous tissue. Haller and Boerhaave opposed the theory of Duhamel; Bichat and Scarpa entirely rejected it. Bichat refused to accord to the periosteum a special property to reproduce bone; he asserted that the formation of the callus depended on the concurrence of all the parts of a fractured bone. Dupuytren partially accepted the theory of Duhamel in attributing an osteogenetic property to the periosteum in the formation of what became classically known as the provisional callus. But the ideas of Bichat and of Scarpa occupied so firmly the minds of surgeons, that previous experiments were ignored and clinical facts rejected, and even the reproduction of bone by every source denied. Ollier remarks that Larrey, in 1818, wrote that everywhere in France surgeons had renounced the errors of Duhamel. Such was the force of dominant opinions, established by the authority of a great name like that of Bichat, that it required the indefatigable efforts of Flourens and many others to reëstablish the discovery of Duhamel concerning the property of the periosteum to produce bone.

Sédillot is, at the present day, one of the most zealous champions against the theory of the reproduction of bone by the periosteum, when this membrane has been detached and isolated from the subjacent bones, as in necroses. He has indulged in considerable warmth of expression, which somewhat lessens the weight of his opinions. At least, we should like to have seen a more impartial spirit of criticism pervade his valuable work, which shared, with that of Ollier, the great prize of the Academy of Medicine. In a review of sub-periosteal resection in his recent work,[1] after alluding to Brunninghausen's recommendation to preserve the periosteum, to cover the ends of bones in amputations, he remarks that, "notwithstanding these negative results and some ineffectual attempts during the second half of the last century, and renewed with as little success about twenty years ago, numerous surgeons of our time have proclaimed with enthusiasm impossible cures, have multiplied the miracles of osseous reproduction, which they alone have observed, and have succeeded in influencing opinion," etc.

In condemning sub-periosteal resection, he speaks of it as a useless and dangerous operation, and claims a positive superiority for his own operation known as "*l'évidement des os*," or scooping out of the bone.

This is an operation which, I believe, may sometimes be advantageously resorted to. I have seen the good results of a somewhat similar procedure in a few cases. The operation of *évidement*, however, has more particularly reference to leaving only the peripheral portion of the bone. But the following eminently practical reflections by Holmes, in his work on the surgical diseases of children, are peculiarly apposite to the subject of "*évidement des os*," and of sub-periosteal resections: "Prof. Sédillot, in a letter recently addressed to the *Société de Chir.* (*Gaz. des Hôp.*, January 19, 1867), has altogether denied that the periosteum, if left behind isolated, has any osteogenetic property. The cases above related will abundantly show how erroneous M. Sédillot's opinion is in this respect, if it be regarded from a practical point of view. M. Sédillot distinctly excludes the merely speculative view of the case, which would look at it as a problem in physiology only, and would draw a distinction between the periosteum proper, a purely fibrous membrane, and some hypothetical plastic tissue underneath it. For surgical purposes, as M. Sédillot truly observes, both these tissues form only a single membrane ; and his assertion is a very clear and distinct one, viz. : that the bone will not be regenerated unless some of the old bone be left attached to the inner face of the periosteum, which can only be accomplished by his method of *évidement.*"

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[1] De l'Évidement des Os, Paris, 1867, page 211.

Mr. Holmes cites three cases which, he observes, prove clearly the reverse.

He says that "at the earliest period at which the bone can be detached from the periosteum, it may be removed entire by sub-periosteal resection, with good prospect of ultimate regeneration. To say that even in these cases some granules of bone will remain attached to the periosteal sheath is nothing to the point. The question is between total excision and scooping out. M. Sédillot's reasoning, if accurate," observes Mr. Holmes, "would show that in cases such as those figured, pp. 392, 395" (and here Mr. Holmes refers to two exceedingly instructive cases in his work on diseases of children), "the shaft of the tibia must have been gouged away piecemeal, otherwise regeneration would not follow." Mr. Holmes remarks that facts show, on the other hand, that the bone was regenerated, though removed in the manner which M. Sédillot denounces.

The especial property of the periosteum to reproduce bone, first proved by Duhamel, has been repeatedly verified since by those who have had occasion to observe attentively the repair of bone during necrosis. I do not design, therefore, to insist particularly upon the reproduction of bone by the periosteum during the eliminative process of a sequestrum. The eminently illustrative cases which have been recorded, in which the continuity of a bone destroyed by necrosis has been manifestly restored, through the agency of the enveloping periostic tissue, would seem to be quite sufficient to accord always to the periosteum the principal part in the reproduction of bone. Such, indeed, is the case, and experiments on animals are confirmed by the clinical experience of a great number of surgeons. Prof. James R. Wood, of this city, unites a large clinical experience of this subject with a rare ability for observation. He recently showed me a portion of rib reproduced by sub-periosteal resection. This operation for necrosis of a rib has a classic history, being done for the first time by the Russian surgeon Karawayew, in 1839, and a little later by Textor.

Innumerable instances, however, of the sub-periosteal resection of different bones for necrosis have been published; and, in 1859, a sub-capsulo-periosteal resection of the elbow-joint was first performed by Verneuil.[1]

The periosteum does not possess the exclusive property to reproduce bone. The bountiful provision of Nature, in its adaptability, acting through the agency of minor laws which exist under the influence of modified conditions, calls into active existence in other tissues the osteogenetic property. The medullary tissue of bones, which possesses to a certain extent this property, thus becomes a more active agent of osseous reproduction. Cellular tissues, fibrous, and even muscular tissues, under the influence of repeated irritation, become ossified. This occurs independently of the influence of the periosteum, except, as Ollier has shown, through an action sometimes of contiguity or action of presence of the periosteum, which membrane seems to occasion a peculiar tendency to ossification in surrounding parts.[2]

The osteogenetic property of the medullary tissue would appear to depend largely on its vascular plexus, which is supported by the oily-like constituents of this tissue.

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[1] De la Resection Sous-Capsulo-Periostée de l'Articulation du Conde, par le Dr. P. Marduel, p. 12. Paris, 1867.

[2] I have examined a specimen of fracture of the humerus, in the possession of Dr. Dunster, the editor of this journal, which shows at once all these conditions, viz.: true bony growths developing in the muscular tissue, and in the connective tissue adjacent to the seat of fracture. There is also development of bone from the periosteum, from the fractured surfaces of the humerus, and from the medullary tissue. The patient died suddenly, from fatty degeneration of the heart, eighteen days after the occurrence of the fracture.

It is proper, in connection with this subject, to remark that there is considerable doubt expressed about the existence of the so-called medullary membrane, which has been described by most anatomists. This membrane, also called the endosteum, or internal periosteum, cannot be detached as a distinct membrane. Connective tissue exists in the medullary substance, and lines the medullary canal of the diapheses of the long bones, but Kölliker has remarked that it should not, properly, be called a membrane, because not possible of separation as such. But this is not a conclusive argument against the existence of such a membrane, because its extreme tenuity would oppose its facile separation from the bone. Bichat rather inferred than proved that such a membrane existed. Flourens attributed to it the function of absorption, by which the medullary canal was increased. MM. Gosselin and Reynauld,[1] twenty years ago, would seem to have proved that the medullary membrane is somewhat of an anatomical myth.

A very important fact in connection with the periosteum is, that a bone denuded of this membrane does not necessarily become necrosed, but may be, and often is, recovered with a new periosteum. It is especially so in the case of a young subject; the denuded surface retains an intimate vascular connection with the medullary tissue, which possesses an inherent bone-producing property, as well as the periosteum. The recollection of this fact should determine the action of the surgeon in certain cases of gunshot and other wounds of bone, in the removal of bone partially denuded of its periosteum.[2] The property of the medullary tissue to reproduce bone is normal, and subserves an admirable purpose in the repair of fractured bones. This tissue is secondary only to the periosteum in its importance as an agent of osseous reproduction.

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[1] See Arch. Gén. de Méd., 4^{me} serie, vol. xx., 1849. Recherches sur la Substance Médullaire des Os.

[2] See an interesting article in Richmond Medical Journal, vol. i., No. 1, by Prof. E. S. Gaillard, on the "Relations of the Periosteum to Osteogenesis." See also the opinions of Ollier, *op. cit.*, vol. ii., p. 123.

While the bone generated by the periosteum is nearly always permanent, that produced by the medullary tissue invariably disappears by absorption. Like the periosteum, the medullary tissue, when transplanted, produces bone. Some important researches recently made by Goujon[1] show that Ollier was wrong in not attributing to the medullary tissue a more inherent and peculiarly osteogenetic property than that he accorded to it. Goujon states that he obtained a reproduction of bone from the transplantation of the medullary tissue six times out of thirty attempts. It sometimes underwent fatty degeneration, and at other times entirely disappeared by absorption, and without inducing suppuration.

In one of his experiments new bone was observed three months after the transplantation of the medullary tissue. He remarks that it is important not to wait too long before examining the product of the transplanted tissue, because the osseous production thus obtained ends by being absorbed. He proffers the explanation for Ollier's failure to obtain a reproduction of bone by transplanting the medullary tissue, that the animals were examined too long after the transplantation. He observes that it is very important to note that there are many more chances of succeeding when these experiments are performed on young animals, and the medullary tissue is transplanted in a very vascular part, as in the middle of a muscle. He states that it is certain that less frequently the medullary tissue is ossified than the periosteum; that the medullary substance is a delicate tissue, soon altered and more easily absorbed than the periosteum. We know that the large multinucleated cells of the medullary tissue, or *cellules à myéloplaxes*, undergo rapid disintegration.

Goujon makes the important observation that, in young animals especially, there is relatively a large number of calcareous salts in the medullary substance.

There are many recorded cases which show that osseous deposits sometimes occur in muscles. It is only recently that extensive ossification of the muscular system has attracted marked attention. In this disease, which is exceedingly rare, sometimes a large number of the muscles of the chest, upper or lower extremities, are successively invaded by an osseous deposit. In the *Archives Générale de Médecine* of last November is an extended review of Münchmeyer's[1] account of this remarkable disease, known as "progressive ossification of the muscles." Mention is made, in the *Bulletin de la Société de Chirurgie*, of 1869, of a case of ossification of the iris, discovered during an attempt at iridectomy, the diagnosis being subsequently confirmed by microscopic inspection. In cases of chronic pleurisy there sometimes exist what are known as costal osteophytes or deposits of bone-substance on the internal surface of the ribs.[2] There is formed in these cases a double rib.

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[1] Recherches Experimentales sur les Propriétés Physiologiques de la Mœlle des Os, par E. Goujon. Journal de l'Anat. et de la Physiol., No. 4, 1869.

Follin, in 1850,[3] called attention to the presence of osteophytes in the pelves of women who had died in childbirth.

The ossification of tissues which normally are not ossified may be the result of chronic inflammation. From this cause deposits of calcareous salts are observed in the cellular tissue. But this is not always a true ossification; however, sometimes the processus is a mixed one, and there exists a partial true ossification, which is characterized by the presence of osseous lacunæ and their canaliculi. Rheumatism is frequently a cause of the ossification of ligaments or articular cartilage. Osteophytes of the dura mater are occasionally observed after syphilis, and are said to be seen after the puerperal condition. Ossification of the pleura and pericardium is occasionally seen.

Traumatic causes or mechanical irritation may give rise to an accidental ossification. Mention is made of a tracheotomy-tube, which was left for several months in the trachea of a child operated upon for croup, in which case ossification of the tracheal rings occurred.

Rayer produced ossification of tendons in animals by leaving pins in them.

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[1] Ueber Myositis Ossificans Progressiva. Zeitschr. f. Ration. Medicin 1868, xxxiv., 1.

[2] Arch. Gén. de Méd., 1849, vol. xxi., p. 320. De l'Ostéophyte Costal Pleurétique, ou Recherches sur une Altération Particulière des côtes dans la Pleurésie. Par le Doct. J. Parise.

[3] Arch. Gén. de Méd., 1850, vol. xxii., p. 251.

The ossification of tissues which do not properly belong to bone is an abnormal condition; in other words, it is an irregularity due to irritation. The presence of an irritated bone may in this manner predispose to ossification of tissues which do not possess normally an osteogenetic property.[1]

The accidental ossification of tissues, which normally do not produce bone, occurring under the influence of irritation, naturally leads to the establishment of an important pathological law, viz.: *that irritation develops more largely the osteogenetic power in bone-tissues proper, such as the periosteum, medulla, and bone-substance, and exceptionally develops this power to a greater or less degree when there exists a perverted assimilation of the calcareous salts of the blood.* All the conditions which modify this assumed law are not well known, and cannot, therefore, in the present state of our knowledge, be accurately determined. To avail ourselves of the conclusions naturally suggested by grouping and assimilating facts acquired by experiment and by observation, it would appear almost needless to say is a necessary and supplementary part in the establishment of all biological laws. And such laws, in a more advanced state of medical knowledge, will, we believe, be the means of pointing to new discoveries and to new resources in the treatment of disease. It is true that a great number of well-observed and accurately-recorded facts are necessary to establish a law, and such work requires an intellectual activity ill-suited to routine methods of thought. Without wishing to assume too much, I will venture to assert that a proper effort has not been generally made to coördinate the facts which we already possess. These reflections are suggested by the impression that there is a growing tendency recently, on the part of certain able English writers, to subordinate facts more to general laws. The study of the conditions of bone-growth in displaced periosteal flaps, I believe, when this subject has been more carefully studied, will afford grounds for an important generalization, which will be found useful in surgical practice.

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[1] This is, undoubtedly, the explanation of the growths of bone in Dr. Dunster's case, before alluded to.

The formative process which produces separately, as a final product, fibrous, muscular, cellular, or other tissues, under changed conditions, as we have seen, such as irritation, undergoes a remarkable modification. This modification is sometimes the result of a repeated irritation, such as occurs after the fracture of a bone which is partially consolidated. The irritation causes an excess of osseous tissue, which subsequently is not wholly removed by absorption.

If, however, there is only a feeble reproduction of bone in detached periosteal membranes, from whatever cause, the conditions of development of such osseous tissue may be incapable of giving it a permanent or prolonged existence.

In such a case the nutritive functions of other tissues in the immediate locality so far predominate over those of the feeble osseous elements that their typical features are destroyed. In other words, the struggle of the different histological elements is proportional to their vigor. We must not be astonished, therefore, that ossification occurring under such circumstances should after two or three months entirely disappear by absorption. Irritation alone will not occasion ossification in parts not normally susceptible of being changed into bone; but there must also exist an elective but perverted assimilation to the part irritated, of those materials of the blood which form bone-tissue. The formative process in tissues which by their contiguity only are related to the periosteum, but otherwise foreign to it, may become, to a limited degree, so changed as to be supplementary and correlative of the inherent bone-producing property of the periosteum. That is, other tissues in the vicinity of bone may produce bone under the influence of irritation. We find, as we have before mentioned, in the reproduction of bone, as throughout nature, the operation of minor causes which more or less favorably modify the principal cause. There must not, however, be too great a departure from the conditions of a certain form of cell-development to produce osseous or other tissues. When we examine microscopically the granular substance of the periosteum of a young rabbit, we see nothing to denote the bone-producing property of this membrane. All we know at present is, that it possesses this property in a most inscrutable manner.

The cellular or fibrous tissue by modifications of nutrition may contain osseous deposits, but generally the essential conditions for this are, that the parts shall be irritated and in contact with the bone, or, as Ollier observes, have certain relations of presence.

Huxley[1] has well observed that "the difference between the vital powers of the lowest plant and highest, or between plants and animals, is one of degree, not of kind, and depends upon the extent to which the principle of the division of labor is carried out in the living economy. In the lowest organisms all parts are competent to perform all functions, and one and the same portion of protoplasm may successively take on the function of feeding, moving, or reproducing. In the highest organisms, a great number of parts combine to form each function."

The eminent microscopist, Dr. Lionel Beale, has opposed the views of Huxley, concerning the protoplasm, but there is reason to believe that Huxley has been misunderstood.

Publications by Flourens, Heine, Textor, Syme, Blandin, Wagner, Ollier, Larghi, and others, by calling attention to the importance of the periosteum as an osteogenetic membrane, suggested the possibility of its transplantation in man with a view to the reproduction of bone. The attempt to transplant the periosteum has been successfully essayed by Ollier and others on animals, it retaining a connection with the bone, or being entirely separated and transferred to another part of the body. Such completely successful results have not yet been seen in the human subject. It has been in certain selected cases, only where favorable conditions have been observed, and the periosteum has retained its connection with the bone, that ossification of the displaced periosteal flaps has taken place. The cases to which I especially refer are those after the operation known as muco-periosteal uranoplasty, an operation for cleft of the hard palate. My experience of this subject, when there has been reproduction of bone, has already been published; and I should not refer to it again, but for the small number of established facts of ossification in man of displaced flaps of the periosteum. I feel convinced that the application of Ollier's experiments on animals cannot fail ultimately to exercise an important influence on the progress of surgery. But the acceptance of a scientific truth is often slow, and necessitates the gradual overthrow of existing prejudices and errors; and it is probable that considerable time will elapse before the labors of Ollier will be fully appreciated by many.

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[1] Fortnightly Review, June, 1869.

There exists, then, but a limited experience of the bone-producing property of the healthy periosteum, when detached and displaced, for the purpose of fulfilling a surgical indication. This limited experience, and the negative results which have most commonly resulted, induced Sédillot (*loc. cit.*, p. 223) to denounce, in strong terms, attempts to obtain ossification of the displaced periosteal flaps in cleft of the hard palate. But nevertheless, the facts exist notwithstanding their denial, and are, though few, sufficiently patent to command attention.

The periosteum, even if we succeed at great pains and trouble in detaching it, is often incapable of reproducing bone, or possesses this property to so small a degree, that the bone-elements disappear either by absorption or are destroyed by excessive suppuration.

Usually, attempts on the dead subject to detach the periosteum are found to be very difficult. The difficulty has resulted from an injudicious selection of instruments, from a defective procedure, and especially from having too little regard for the age of the subject, and the locality selected for the experiment. Ollier[1] remarks that, before the age of ten, it appears impossible to make other than a sub-periosteal resection. This, he states, seems paradoxical, but is nevertheless an anatomical consequence; the adherence of the periosteum to the soft parts being greater than to the bone. He observes that, if the femur or humerus of the cadaver of a child be broken, it is impossible to make the bones project, without being deprived of their periosteum; and he cites cases observed in the living subject confirmatory of this experiment. Ollier mentions a case reported by Dr. Fabre,[2] of a lad six years old, who had a fracture of the femur, with projection of the bone. It was excised for a distance of seven centimetres or 2¾ inches. Proper extension and counter-extension were made during the treatment, and a cure resulted without any shortening; and there was a firm osseous production in the place of the resected bone.

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[1] *Op. cit.*, vol. ii., page 62.

[2] Idem., vol. ii., page 63.

The periosteum is quite variable in thickness and vascularity, according to the age of the subject and the proximity to the epipheses of the long bones. While this membrane is thin, and especially difficult to be separated from near the central part of the diaphesis of any of the long bones of an old subject, below the age of fifteen years the periosteum may be detached with great facility, on observing proper precautions. The greater vascularity of the periosteum in children than in adults corresponds with a greater vascularity of the bone-tissue during childhood.

It is interesting to note that the tenuity and diminished osteogenetic property of this membrane coincide likewise with a greatly-increased growth of the intercellular bone-substance. This is sufficiently apparent from the recent experiments of Dr. Carl Ruge,[1] of Berlin. The intercellular bone-substance, he has shown, by repeated direct measurements, increases with the age, or, in other words, the spaces between the osseous lacunæ become enlarged in all directions by the deposit of earthy salts, which cause a gradual intercellular growth of the bone. Its vascularity is, of course, thus correspondingly diminished, because the number of osseous lacunæ are relatively to the increased volume of the bone less numerous. So the above fact suggests two reasons why the osteogenetic power of the periosteum in an old subject is lessened; one of which is, that this membrane is thinner, as well as less vascular; and the other is, that the correlative bone-reparative property of the bone itself is also diminished.

I can fully testify to the great facility with which the periosteum may be detached from its connections with the bone, in the operation for cleft of the hard palate. The irregularities of the surface of the alveolar processes, and horizontal plates of the maxillæ, would at first sight suggest extreme difficulty in detaching the periosteum in this operation. But such is not the case; the muco-periosteal envelope of the roof of the mouth forms a thick and vascular membrane—the periosteum being so intimately united to the mucous covering that it may be said to form with it a single membrane. Langenbeck has reported, as is known, cases of reproduction of bone in these displaced flaps of the periosteum, after the operation of uranoplasty; and my own cases are, it seems to me, sufficiently conclusive to entirely confirm the statement of Langenbeck. The tests used in these cases are detailed at length in my Report on Cleft Palate,[1] read before the American Medical Association. The difficulty of detaching the periosteum, under certain unfavorable conditions, being understood, the question occurs: Will the periosteum, when removed from the bone, and having no connection with it, reproduce, when transplanted to a distant part, new bone? This question is difficult of solution in animals, and osseous regeneration under these circumstances has been repeatedly observed by a number of European experimentalists. According to Ollier, in some animals, such as the sheep and calf, which are supposed to approach nearer to man than the dog or cat, the transplantation of the periosteum does not succeed so well as in the last two. The facts recorded by Ollier concerning the transplantation of the periosteum in animals, especially in the rabbit, are numerous and interesting.

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[1] Ueber cellulares und intercellulares (Sog. interstitielles) Knochenwachsthum. Virchow's Archiv., January, 1870.

I have found it exceedingly difficult to isolate and transplant the periosteum of young rabbits. This membrane, on the middle of the tibia of a rabbit three months old, is as thin as a spider's web, and, though quite easily separated from the bone, because more adherent to the adjacent soft tissues than to it, is most difficult to isolate. The few experiments of this sort which I have attempted, thus far, have not been altogether satisfactory. Greater precautions and more experience in making them are required. But the following experiments, showing the reproductive properties of the healthy periosteum, are deemed of sufficient interest to be cited:

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[1] See Transactions of American Medical Association, vol. xx., 1869. Report on the Best Methods of Treatment for Different Forms of Cleft Palate.

Experiment I.—January 12, 1870. From a young rabbit, about six months old, two and a half inches of the right ulna were removed. There was considerable laceration of the soft parts, and much of the periosteum was removed with the bone. The experiment was badly done, the metacarpal articulation being unnecessarily opened in two places. The wound was sutured with silver wire, the leg bandaged and splinted. The rabbit recovered rapidly, and the following specimen, which I present to the Society, shows a partial substitution of new bone for that removed. There is extensive reproduction at the distal portion, near the epiphysis, with a projecting point extending upward and inward. The reproduction is equally well marked from the point of section of the bone, and is a stout osseous cone, gradually tapering to a point, and closely applied to the inner surface of the radius.

Experiment II.—I removed from a rabbit of the same age as the preceding one, and on the same day, two inches of the right ulna, cutting directly down on the bone in the intermuscular space. The periosteum was far more adherent to the soft parts, except at the epiphysis, than to the bone. The bone was cut with a stout pair of sharp-pointed scissors in the middle, and two pieces removed. At the carpal end of the distal piece the bone was cleanly separated from the incrusting cartilage. This rabbit cried lustily twice, once in separating the periosteum, but not at all when the bone was crushed in cutting it with scissors; and it cried again on clipping its ear, to distinguish it from the others. Wound was closed with thread sutures, leg bandaged and splinted. About two months afterward the rabbit was killed, and the following specimen, marked No. 2, which I show you, exhibits a complete reproduction of bone; but the new bone is very small, except at its extremities, where it is larger than at those portions of the bone which was removed, and which will permit you to compare the new bone with that removed.

Experiment III.—From a strong, stout rabbit, a year old, two and a half inches of the ulna were removed. The periosteum was very adherent to the bone, and it was found impossible to remove it, except in very small threads. The animal was considerably mutilated, but gave no evidence of pain, either while cutting the bone or soft parts. Wound closed and sutured, and leg splinted. There was not the slightest sign of reproduction of bone, and specimen No. 3 shows you the extent of the resection, and the size of the resected bone.

Art. IV.—*Pathology and Treatment of Urethritis.*[1] By Alexander W. Stein, M. D., Professor of Physiology and Histology in the New York College of Dentistry, and of Comparative Physiology and Histology in the New York College of Veterinary Surgery.

The disease which I purpose to consider has been known by a variety of names. Originally, the discharge in this affection was thought to be semen, from whence the term gonorrhœa (γονη, sperm, and ρεω, to flow) originated. This term, strange as it may seem, is still almost universally retained. Subsequently, the name blennorrhagia was substituted, signifying (βλεννα, mucus, and ρεω, to flow) a discharge of mucus. Among the French, it is popularly called *chaude-pisse*, from one of its characteristic symptoms. In Germany, they call it *tripper.* In English, it is familiarly known as clap. It is unnecessary to indicate in what particular these terms are unsatisfactory. I merely mention them to show that we are still in want of an appropriate name for the disease. If, however, we are to make a choice from the above terms, blennorrhagia recommends itself as, perhaps, the most comprehensive, being applicable to the disease in either sex, with the reservation, however, that by it we mean not only a discharge of mucus, but a fluid mixture, composed of mucus, pus, epithelium, and perhaps a few blood-corpuscles. At present, however, my remarks are confined to the disease as it occurs in the male, and here we have no difficulty in finding a name which fully indicates the seat and character of the disease. For it matters not whether the disease arises from a so-called "specific" or other cause; its symptoms, nature, and course, remain the same. It is, in brief, a *urethritis.* Urethritis, as its name implies, is an inflammation of the urethral mucous membrane, characterized by a profuse muco-purulent discharge from the meatus, and accompanied by the usual inflammatory symptoms, heat, pain, redness, and swelling. Allusion to this disease is so distinctly made by the Hebrews, Greeks, and Romans, that there can be no doubt that it was well known to them. Still, even if we possessed no historical data respecting the existence of this affection in those early times, an appreciation of the causes which produce it is sufficient to give us positive evidence of its having existed during the earliest periods of the history of man. It is, by far, the most frequent disease arising from sexual intercourse, and yet, considering the many and apparently slight causes which may produce it, it is quite surprising that it does not occur more frequently. The causes of urethritis are generally considered under two heads: predisposing, and exciting. It is a fact well established that there are certain constitutional conditions which, more than others, render an individual predisposed to inflammatory affections. Among such are usually mentioned the rheumatic, arthritic, or scrofulous diathesis, and lymphatic temperaments. These are also recognized as constitutional causes of urethritis. There can be no doubt that certain individuals are more susceptible in contracting urethritis than others. This tendency is shown not only in the fact that one individual will contract the disease when another will escape—although similarly exposed—but, also, that with some the inflammation is more intense, persistent, or recurrent, than it is with others. A striking example of this occurred, last spring, in my own practice. Two of my students, both healthy young men, neither of whom ever had urethritis, cohabited with the same female the evening after commencement night. One contracted a urethritis of rather an intractable nature, while the other entirely escaped. It has been observed that a female, having intercourse with several men successively, will be more liable to induce inflammation in the last than in the first. However true this statement may be, which in itself is very reasonable, it has no bearing upon the case in point, for I was informed by the gentleman who received the prize that he was the first contestant. I think it is true that the more frequently a mucous membrane is affected with inflammation, the more susceptible does it become to subsequent exposure. This, at least, is a fact with reference to the urethra. Those who have previously suffered with urethritis are more than ordinarily predisposed to renewed attacks. This is, perhaps, owing to the fact that the mucous membrane is, to some extent, weakened by the previous inflammation, and is thus texturally predisposed. The subsequent inflammations are less active and the symptoms less severe, though they are, as a rule, more persistent in their duration and more difficult to manage.

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[1] This paper was read before the New York Medical Journal Association, February 4, 1870.

Special excitement during sexual intercourse, prolonged intercourse, free indulgence in stimulants, etc., these have all been classed under the head of *exciting* causes. They are certainly strong *predisposing* causes; but I doubt indeed whether either of them is sufficient, in the absence of some irritating secretion, to induce a very active urethritis, with muco-purulent discharge, in an individual who has not the remains of a previous urethritis, or in one who has never had the disease. I am aware that cases have been reported, by competent observers, of urethritis contracted from a female in whom, upon examination, nothing abnormal was found. This may be accounted for by the fact that the ordinary uterine and vaginal secretions, when allowed to accumulate for some time, become highly acrid and irritating, and I think it not improbable that urethritis is sometimes excited in this way. Before speaking of exciting causes, I desire to allude to another condition which I believe is not an infrequent predisposing cause. I refer to an exhausted condition of the system from defective nutrition or otherwise. We know how readily an ordinary catarrh or cold is contracted after a fatiguing journey, during convalescence from some exhausting disease, or during any weakened condition of the body. The exciting causes may be enumerated as follows: first, traumatic; second, direct transmission; third, all irritating vaginal or uterine discharges.

The injudicious use of instruments, application of irritating drugs, etc., are occasional causes of urethritis; but by far the most frequent cause is sexual intercourse. That urethritis is often the result of direct transmission of the disease by a woman similarly affected, there can be no question, but that this is the only or even the most frequent manner of contagion has long since been refuted. It is of the greatest importance that we should fully appreciate this fact, for the most disastrous consequences have resulted from ignorance upon this point. It cannot be too often mentioned, that urethritis is not always the result of illicit or impure intercourse. The observations of Ricord, Diday, Fournier, Sir Henry Thompson, Henry Lee, Bumstead, Hill, Skey, and, in fact, nearly all modern investigators upon this subject, have long since established the truth that urethritis is frequently the result of sexual intercourse with a female during the existence of a discharge not venereal in its origin.

A short time ago, pathologists claimed, with the greatest assurance possible, their ability to determine the difference between malignant and non-malignant growths; and now, after many years of controversy upon this point, they are obliged, notwithstanding the recent improvements in optical instruments, to admit the extreme difficulty, and in many instances the impossibility of determining with any degree of accuracy whether a tumor belongs to the malignant or non-malignant variety. So, also, with regard to the discharge in urethritis, much has been said concerning the distinctive characters between the discharge of the so-called "specific blennorrhagia" and that of "simple urethritis;" and repeated attempts have made to discover something in the former which did not exist in the latter, and by which it might be distinguished. But these investigations have all proved futile. The only criterion we at present possess, by which we can determine the contagious character of a discharge, is by its containing pus. Every discharge containing pus, when brought in contact with a membrane similar in structure and susceptibility, may be transmitted. The following I take from Holmes's "System of Surgery," vol. iv., p. 632: "The fact of the disease being so commonly caused by impure sexual intercourse, is proof of the presence and action of a poison, *but only of such a one as is capable of being produced by simple inflammation.*" Opinions have been much modified of late concerning the "specificity" of inflammations. Van Roosbröck's experiments go to prove that an ophthalmia may result from an inoculation of *any pus*, and that the true morbific property of that fluid depends upon the *cells;* for, when it has been deprived of such by filtration, the remaining liquor appeared innocuous.

Mr. Simon, in his essay upon inflammation (vol. i., p. 69, Holmes's Surgery), remarks: "There is ample room to question the popular impression that only *specific* inflammations are communicable—much reason for suspecting it, on the contrary, to be a generic and essential property of inflammation; that its action (or some of them) are always in their kind, to some extent, contagious. Mr. Simon quotes from Dr. Gaillies's and Sir Patrick McGregor's experience upon the occasional contagious properties of infantile and common catarrhal ophthalmia, and cites Dr. Piringer's experiments to the effect that he had succeeded eighty-seven times in exciting conjunctival inflammation by the contact of an inflammatory product (pus) taken from sources most various." From these data, therefore, it is not surprising that the various discharges from the genitalia of the female so frequently occasion a similar discharge from the urethra of the male.

All writers agree that intercourse with a female during the menstrual period is a fruitful cause of urethritis. This is certainly true, but I think the impression is very often conveyed that something specific or *sui generis* exists in this (menstrual) fluid. If we investigate the matter, however, we shall find here a condition similar to that which obtains in ordinary leucorrhœa. Most women about the time of the menstrual period have some local inflammation of the mucous membrane of the uterus, with accompanying discharge, and it is this discharge, mixed with the blood, that is really the exciting cause, and not the blood *per se*, which contains nothing more contagious in it than blood derived from other parts of the body. Appreciating, then, the general pathology and etiology of the disease, we are forced to the conclusion that there is nothing more "specific" in the discharge of urethritis than in that of any other inflamed mucous membrane. However minute the examination, nothing but the usual products of catarrhal inflammation as found elsewhere can here be detected. Indeed, it may be asserted that this affection is in all respects identical with other catarrhal affections. It is absolutely a local disease, excited by some irritation of the mucous membrane. No matter whether this irritation has been induced by the abuse of instruments, leucorrhœal discharge, or any other irritant, the discharge resulting remains the same, and can in no way be distinguished from, and is as readily communicated as, that arising from the so-called "specific contagion" or direct transmission.

In the beginning, the inflammation is limited to the anterior portion of the urethra or fossa navicularis; but, as the disease progresses, it gradually extends to the deeper portions of the canal, and not unfrequently as far as the bulbous portion. At this point it generally subsides, and ends either in resolution or chronic inflammation, or that condition generally known as gleet or granular urethritis, which, if not judiciously treated and removed, will inevitably lead to the formation of organic stricture. In some active cases, however, the inflammation does not stop at the bulb, but travels on by continuity of tissue, even to the neck of the bladder, or through the ejaculatory ducts to the epididymis, involving either one or both sides in inflammation. This is rather an unfortunate complication, generally ending in temporary, and often permanent, obliteration of the duct, and, if it affects both sides, produces sterility. M. Liegeois has observed that, even when the affection is unilateral, it induces sympathetically a diminution in the quantity of spermatozoids furnished by the healthy testicle, and thus "seriously influences the fecundating properties of the seminal fluid." Endoscopic examinations are so extremely painful during the acute stage, that they should not be attempted. In the stage of decline they produce little inconvenience if the instrument is gently manipulated. I have made many such examinations with the object of finding the long-looked-for ulceration of the mucous membrane, but with no success. All that can be observed is a highly-congested mucous surface. One fact, however, these examinations have often demonstrated to me, namely, the tendency which the disease has to lurk in the vicinity of the bulb. I have found this part of the canal the seat of chronic inflammation when, both anterior and posterior to it, the mucous membrane was apparently healthy.

In order correctly to appreciate the source of the discharge, allow me briefly to review the theories entertained since 1838 respecting cell-growth. In that year Schwann announced his theory of spontaneous generation, or the development of cells out of a formative fluid or blastema. This view was generally accepted, and more or less steadfastly adhered to until 1852, when Rudolph Virchow, and subsequently L. S. Beale, of London, boldly came out with an exposition of the error of Schwann, refuting entirely the blastema doctrine and substituting the doctrine of "omnis cellula e cellula." They claim it to be impossible for any cell to develop *de novo*, or that a cell can ever build itself out of any non-cellular substance. Every cell is derived by direct descent from a preëxisting cell, by proliferation. Virchow says: "However much the different surfaces presented by the skin and mucous and serous membrane are examined, the conviction is everywhere unmistakably acquired that the cellular elements extend down to the very surface of the connective tissue, and that there is nowhere a spot, where free nuclei, blastema, or fluid exists, but that, on the contrary, it is especially the deepest layers which contain the most densely-crowded cells." Furthermore, it has been long since demonstrated that mucus and pus-corpuscles are never produced spontaneously in an exuded fluid or blastema, as was at one time assumed, but are continuously descended from preëxisting cells. They are really nothing but imperfectly-developed epithelial cells, or, as Virchow graphically remarks, "They must be looked upon as epithelial cells that had gone astray, as children that had turned out ill, that had been impeded in the progress of their development by some early disturbance, but really were intended to become epithelial cells." Inflammation always presupposes some kind or degree of irritation. The young epithelial cells are excited or stimulated to increased activity, enabling them to attract, absorb, and transform more nutritive material than is actually required for their normal maintenance and growth. Conversion of pabulum, growth, and proliferation, now rapidly follow in succession, and so rapid is this change that there is no time for the gradual development of the normal epithelium. The cells lose their formative power, and become as it were degenerated into pus. This is briefly the manner in which the discharge originates.

This leads me to refer to another interesting point in connection with the pathology of urethritis, namely: the difference between mucous surfaces with regard to the formation of pus. In some suppuration is always associated with ulceration, while in others this does not occur. For example, when pus is formed upon mucous membrane supplied with cylindrical epithelium (as in the intestines), a breach of surface or ulceration can almost always on close inspection be found; while in those provided with stratified epithelium (anterior portion of the urethra) this never occurs. This fact is probably explained by the anatomical relation which these epithelial elements bear to the underlying connective tissue. Where cylindrical epithelium exists, it is often impossible to draw a line of demarcation between the çell and underlying connective tissue; and hence pathological changes commencing in the cell often extend to the connective tissue, involving it in the condition known as ulceration. In the stratified variety, however, the several layers are clearly defined, and suppuration is entirely confined to these elements; the connective tissue is in no way involved. This observation is well exemplified in the urethra. The anterior portion of this canal, from whence the discharge is principally derived, is very largely supplied with stratified epithelium, and there is perhaps no other mucous membrane in the body of such limited surface which furnishes such an abundance of purulent matter as this when inflamed. Yet, according to the investigations of Hunter, Sir Astley Cooper, and Ricord, who made recent dissections upon subjects who died while suffering from urethritis, no breach of surface could be found. The evidence collated is at least sufficient to show that ulceration of the mucous membrane is very rare, especially in *anterior* portion, and my own observations with the endoscope give to my mind additional evidence of the fact.

It would be an endless and unprofitable task for me to attempt to enumerate the various modes of treatment which have been suggested from time to time by different writers. I shall confine my remarks principally to that mode of treatment which experience has led me to adopt as the most rational and successful. The treatment of urethritis is usually considered under two heads—abortive and curative. The advocates of the abortive method claim that active treatment, instituted before the acute stage has fairly commenced, will often materially cut short the duration of the disease. The plan generally adopted consists in the injection of argenti nitras, with a view of substituting artificial inflammation. With regard to this treatment, I can only say I have very little to do with it:

1. Because it is extremely seldom that I see my patients when such a treatment is justifiable.

2. That when the opportunity offers it very frequently fails in its object.

3. When it fails it intensifies and prolongs the disease; and

4. Because of the danger of producing a good deal of mischief. Furthermore, it is not improbable that many of the cases reported as successful instances of this practice would have recovered without going through the painful ordeal of caustic injections. Slight irritation at the meatus with a little mucous discharge is not an unfrequent consequence upon excessive sexual intercourse, which usually disappears within thirty-six hours under very mild treatment, often with no interference whatever. These slight symptoms, therefore, cannot always be regarded as indicating abortive measures.

Of all remedies, the argenti nitras is least adapted for injection into the urethra. If a strong solution of this caustic is applied, it frequently excites violent inflammation. I would urgently guard against the employment of the strong solutions so frequently recommended by authors. On the other hand, very weak solutions are soon decomposed by the discharges, and are therefore inefficient. Other external as well as internal remedies have been tried for the abortive treatment, but thus far have proved equally unsatisfactory. Considering the suffering to which this affection gives rise, its frequent complications and its occasional disastrous sequelæ, it is greatly to be hoped that some more rational and reliable means may yet be discovered by which it may be arrested in its early progress.

When active inflammation has fairly commenced, any attempt to abort the disease is out of the question. We must now have recourse to curative measures. Our first indication is to relieve the inflamed part as much as possible from every source of irritation. This will be fulfilled by rest, attention to diet, use of diluents, etc. The more thoroughly we succeed in keeping our patient at rest and in the recumbent posture, the greater in proportion will be our success in the treatment of his disease. It is also essential to keep the genitals well supported by a comfortably-fitting suspensory bag or bandage, to favor the return of blood from the parts. The universal practice of wearing lint in contact with the meatus to prevent soiling the clothes is to be deprecated; it almost always intensifies the disease, and favors its extension backward by preventing the escape of the discharge. If lint is worn at all, it should be loosely applied, and very frequently changed, and the organ thoroughly washed. The disease being situated in a canal through which one of the excrementitious fluids of the body must pass, and as this fluid contains substances highly irritating to the diseased surface (as indicated by the almost intolerable pain occasioned by micturition), our first object must be to render the urine as bland and unirritating as possible. To this end we direct the patient to drink moderately of some alkaline-water, or administer some of the alkalies, of which I usually prefer the potass., bicarb. grs. xv. every three or four hours, well diluted. To this the tinct. hyoscyami is sometimes added with benefit. Attention must next be directed to the diet. Spirituous as well as malt drinks must be interdicted, not only because the alcohol removed by the kidneys tends to increase the irritating character of the urine, but also, on account of its exciting nature, tending to produce erections, chordee, and indeed increase the severity of the symptoms generally. Equally important in this respect is the effect of the food upon the urine. Nitrogenized articles tend to the production of urea, which substance is very irritating to the inflamed surface. Hence, meats of all kinds should be as moderately indulged in as is consistent with the health and condition of the patient. Spices of all kinds, such as mustard, pepper, vinegar, pickles, etc., are to be avoided. Of chocolate, coffee, and tea, the latter is least objectionable. In fine, the food and drink must be unirritating, highly digestible, and regulated in quantity according to the habits and requirements of each individual case. We must not forget that inflammation may coexist with a state of debility or exhaustion, when the above treatment would in a measure be contraindicated.

In chronic cases especially, the blood is often impoverished from long-continued antiphlogistic treatment. There is want of appetite, general *malaise*, etc. These cases will not bear depressing treatment. We must endeavor by generous diet, tonics, and even stimulants, to improve digestion, the condition of the blood, and the nutrition of the body generally before we can expect to cure the local disease.

The bowels are to be kept free, in order to relieve the hemorrhoidal vessels from pressure. For this purpose, mild laxatives may occasionally be necessary. The use of tobacco in every shape tends to keep up the discharge, and must therefore be rejected. The pain in making water is often greatly alleviated by immersing the organ in ice-cold water, as recommended by Berkley Hill, or by injecting a little ice-water immediately before micturating. The sedative injections recommended in this stage have not proved very satisfactory in my practice, and I therefore prefer the cold water above mentioned. The practice of wrapping the organ in wetted cloths and leaving them on for an indefinite time, is worse than useless. If cold lotions are used at all, they must be continuously applied, so as to keep up a uniform low temperature, otherwise they can be of no benefit. Cold lotions are sometimes, however, not well borne, in which case they must be discontinued, and the hot fomentations tried instead. To prevent frequent erections and chordee, the patient must sleep cool and upon a hard mattress. He must avoid eating or drinking before going to bed, and avoid lying on his back. His bladder must be thoroughly emptied, and his bowels free. Among the remedies employed to prevent or allay this symptom, are: lupulin, camphor, potass. bromide, opium, etc. Camphor is said to act as an anaphrodisiac. It is generally given in pill-form or with aqua camph. The following is a very good formula: ℞. camph. ʒj, chloroformi ℥ss., Twenty drops in a wineglassful of milk. In some instances this remedy is of undoubted value, in others it fails. The potass. bromide I have tried in ℈j and ʒ ss. doses, without the desired effect. Much more reliable and rapid is the effect of morphia, either as a suppository of ⅓ or ½ gr. morph. in grs. x. of cocoa-butter, as recommended by Berkley Hill, or a subcutaneous injection into the perinæum. This often allays irritation in a few minutes, and affords rest.

As soon as the urethral irritation is allayed by rest, anodynes, proper diet, cataplasms, diluents, laxatives, etc., we may begin with the anti-blennorrhagics. Among the great variety of anti-blennorrhagics recommended, the most popular and at the same time most disgusting, are copaiba and cubebs. It cannot be denied that both have in many instances an undoubted control over the disease. Yet it is equally true that too often they are altogether inefficient, and in many instances inadmissible. Furthermore, despite all the precautions that can be taken with reference to mode of administration, etc., they frequently produce, to say the least, exceedingly diasgreeable and annoying symptoms. I am glad to say that these nauseous and disgusting drugs, still so much in vogue, are rapidly going into disuse. Dr. Dyes very truly remarks: "It is almost incredible how practitioners, who, throughout the whole course of gonorrhœa, order abstinence from every thing likely to cause irritation, can, in direct opposition to this, prescribe a medicine so irritating to the kidneys and urinary passages as balsam of copaiba. During the first years of my practice I rigorously followed the dictates of my preceptors, and gave copaiba and cubebs in every case of urethritis that came under my charge; but now, after considerable experience with this disease, I am fully satisfied that it does better without than with these remedies. A few years ago, Q. S. Prettyman, M. D., of Milford, Del., recommended very highly the oil of *Erigeron Canadense*, or that from the *Philadelphicum*. He said he had prescribed it in about fifty cases with great success. He usually precedes the remedy with some active hydragogue, as the following: ℞ pulv. sennæ ℈ij, pulv. jalapæ ℈j, pulv. aromat. grs. x., M.; add a gill of boiling water and a teaspoonful of sugar, and, when sufficiently cool, agitate and swallow at a dose. When this has operated, he commences with the oil of erigeron, giving ten drops on a piece of sugar. Three hours after he gives a full dose of spts. æther nitrici in decoct. altheæ. This he alternates with the oil every three hours until the urethral irritation has subsided, when he continues with the oil alone until the cure is complete. This treatment, he says, "arrests the discharge in about seventy-two hours, and effects a cure in from six to eight days." I regret to say that my experience does not confirm this statement. I have tried the erigeron pretty faithfully—notwithstanding that it is sometimes difficult to induce patients to take it—and have not met with any thing like the success attained by Dr. Prettyman. Indeed, I regard its value very limited in this disease. Lastly, I must mention a remedy which has given me more satisfaction than any thing I have hitherto employed. The ol. sant. flav. is more useful and certainly as efficacious as any of the other and more disagreeable anti-blennorrhagics. Indeed, it has often been found successful in cases which had proved rebellious to both copaiba and cubebs. It has further the great advantage that it can be taken in the majority of cases without inconvenience or without disordering the system. It has to many a very pleasant odor, not at all an objectionable taste, and a very soothing effect upon the inflamed urethra. For this reason it can be administered with benefit even while there is considerable scalding and pain. The oil of yellow sandal-wood is obtained by distillation from the wood of the tree *sirium myrtifolium* of the genus *santalum*. It grows in the East Indies. The color of the oil when pure is of a pale-amber tint. The dose is mentioned as from fifteen to sixty drops three times a day, taken with liq. potass., or in peppermint-water, or, which I think is still more preferable, in capsules. I have not found these large doses necessary; from ten to twenty-five drops, three or four times a day, is usually quite sufficient. It has a most brilliant effect in controlling the discharge. Forty-eight hours is often sufficient to suppress a very abundant flow. Mr. Robert Park, of Great Stanmore, who seems to have had considerable experience with it, writes: "Its action in blennorrhagia seems to be quite specific. In fact, if I believed any medicine was really and truly entitled to be called a specific in reference to any particular disease or complaint, I would say this one had earned its title *par excellence*."

I could mention a host of other remedies, some old and some new, which have received the encomiums of the profession, but shall not occupy your time unnecessarily.

Mr. J. S. Milton writes: "What we want is not new drugs, but a more complete knowledge of the power, both *positive* and *relative*, of the more valuable remedies we already possess, and along with this the ruthless elimination of a host of useless drugs which at present only serve to encumber the druggist's shop and the author's pages, to puzzle the brains of those who have not had the ill-luck to use them, and excite the disgust of those who have."

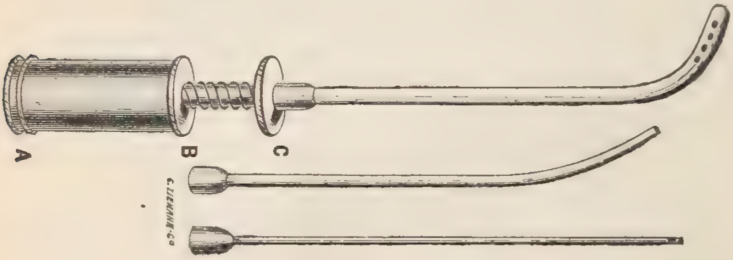
While it cannot be denied that internal medication often exercises a decided influence upon the disease, it is equally true that local treatment is, in the majority of instances, more reliable than remedies taken internally. Much, however, depends upon the time when local treatment is commenced. A too early use of injections will almost inevitably be fraught with evil consequences; while, if deferred until the chronic stage has set in (granular urethra), they are useless. The time when injections may be employed with perfect safety and to the best advantage is immediately after the acute stage has passed over. The object of this treatment cannot be to arrest the inflammation, but to suppress the discharge after the acute symptoms have subsided. Among the numerous remedies recommended by different writers are the preparations of argenti nitras, zinc, lead, copper, iron, tannin, matico, red wine, potass. permanganate, potass. chlorat., acid carbolic, etc. A careful discrimination should be made in the choice of these remedies.

The strength as well as the choice of a local application should be governed as much by the sensibility or irritability of the part to which it is applied as the administration of an internal remedy is according to the idiosyncrasy or susceptibility of the individual to whom it is given. For example, it not unfrequently happens that an injection which acts promptly and effectively in one case, will be altogether valueless in another. Again, Mr. A. will take an injection of a certain strength with no inconvenience whatever, while the same will produce intolerable pain in Mr. B. It is, therefore, sometimes necessary to change the remedy or its strength once or twice until the one adapted to each particular case shall have been found. It is best to commence with a very weak solution, and gradually increase its strength if necessary. The injection must never be strong enough to irritate; as soon as it produces much inconvenience it should be diluted. The following formula will often be found of sufficient strength, especially in the beginning of treatment: ℞. liq. plumbi subacetat. ʒj, aquæ ℥iv. If a stronger solution should be deemed necessary, one to two grains of the zinci sulph. to the ounce, may be added to the above. In the majority of cases this will be quite sufficient. Yet, as I have already indicated, it may be necessary to increase the strength of even this, or substitute some one of the other astringents above mentioned. It has been considered a desideratum in treatment to keep the walls of the urethra apart. For this purpose solid or semi-solid injections have been advised. Bismuth has been used by insufflation, for which purpose a catheter has been especially constructed; and it has also been used suspended in glycerine and water. More recently starch has been employed in a similar manner. Starch is finely pulverized and mixed with lukewarm water or glycerine, so as to form a tolerably thick, creamy fluid. Astringent substances are sometimes added to these powders. I have no experience with this treatment. As far as I can judge, it has not met with much favor. It evidently can only be of service in chronic cases. Recently the glycerine of tannin has come into favor with some. The following is the formula for its preparation, given in the *British Pharmacopœia*, of 1867: "Take of tannic acid, (℥j.) one ounce; glycerine, (f. ℥iv.) four ounces. Rub them together in a mortar, then transfer the mixture to a porcelain dish, and apply a gentle heat, until ample solution is effected." One part of this is to be diluted with about three of sweet-oil. The first effect of this injection is to rapidly diminish the discharge, but this result is not permanent. After a while the discharge returns, notwithstanding the continued use of the injection. It is always necessary to resort to some other injection (as zinci acet. or sulph., two or three grains to the ounce of water) afterward to complete the cure. More reliable than the above is the sulpho-carbolate of zinc. This preparation was first suggested by Mr. Sansom, of London, and prepared by J. Balmer, 205 St. John Street Road, London. I have now used it in a number of instances, and can speak very favorably of its effects. I use it in the strength of from three to ten grains of the crystals to the ounce of water. In this strength it is not at all irritating, and prompt in diminishing the discharge.

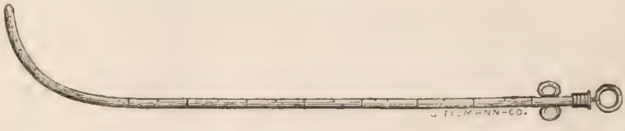
While a judicious selection of remedies is in many instances of very great importance, it must be admitted that success often depends more upon the thorough application of an injection than upon the substance of which it is composed, or its strength. It is, therefore, eminently necessary to instruct the patient, not verbally, but by actual demonstration, how the injection is to be made. The all-important object is to bring the fluid in actual contact with the diseased surface. To this end the patient is directed to micturate, in order to clear out the accumulated discharge. This is best done by compressing the meatus until the urethra is well distended and then suddenly allowing the urine to escape. Thus, not only the urine, but much of the discharge, will come with it. The nozzle of the syringe should now be fully introduced into the urethra. The anterior part of the glans is to be compressed around the nozzle to prevent the escape of the fluid. The injection is now gently and steadily made until the urethra is fully distended, or as much so as the patient can bear without pain. The syringe is now removed, the injection retained for half a minute, and allowed to escape. A second injection is made in precisely the same manner as the first; this should be retained in the urethra from one to two minutes. The thorough application of the injection may be further facilitated by passing a finger of the right hand to and fro along the under surface of the penis. As already stated, the injections should be used only after micturition, and indeed, if possible, after *every* micturition. They should not be used directly after rising, or immediately before going to bed. Early in the morning the urethra is often extremely sensitive, and injections are not well borne at this time; it is well, therefore, to wait until this has passed off. Again, if used immediately before going to bed, they favor the occurrence of erections, which of course are to be avoided.

Occasionally we meet with individuals who are so solicitous about themselves that it is almost impossible, even for some time after all signs of the disease have disappeared, to convince them of their recovery. More frequently, however, patients are too apt to consider themselves cured before such is really the case. As soon as the pain and discharge have stopped, they believe themselves cured, discontinue treatment, indulge in some excess, and very naturally have a relapse.

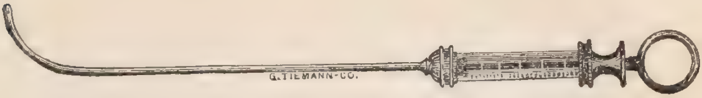
This fact renders it always necessary to impress upon patients the importance of avoiding all kinds of excess, and continuing treatment for from ten to fourteen days after all signs of the disease have disappeared. The injections particularly should be continued for at least ten days after all traces of the discharge have left. They should, however, be gradually discontinued, first the noon, then the morning, and finally the evening injection. In spite of every thing, the disease will sometimes locate itself in the vicinity of the bulb, manifesting itself by a thin, gleety discharge. The recognized difficulty of thoroughly applying an injection by the ordinary means to the entire diseased surface, when deep seated, has led to the production of a great variety of ingeniously-contrived syringes and other instruments.



This instrument, devised, I believe, by Dr. Hutchinson, is especially adapted for ointments, although fluids may be used as well. The cylinder A B is first filled with the ointment at A, and the cap A screwed on, which prevents any of the injection escaping at that point.

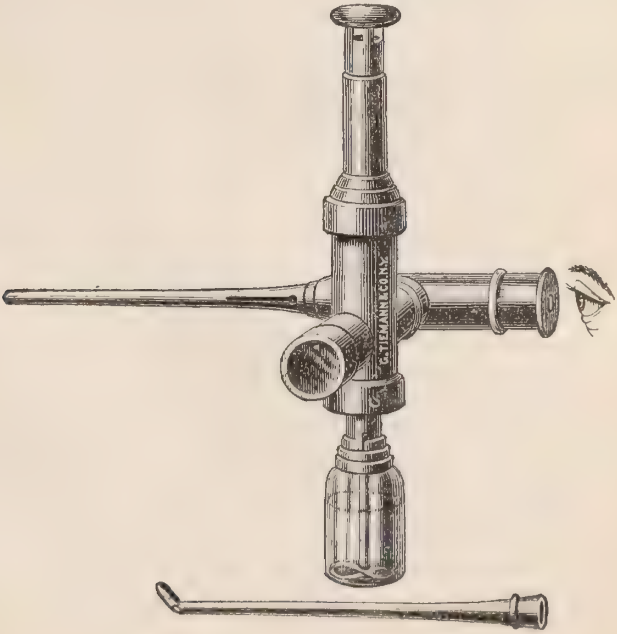


This is a graduated catheter syringe, which may be used as an ordinary syringe, or by compressing a fine sponge at the bottom of the catheter.



This instrument of Dr. Bumstead's is, I think, an improvement on the last. It consists of a graduated glass cylinder to which is attached a hard rubber tube. The advantage of this instrument is that the piston can always be made to work air-tight. It can be used with one hand and for either fluids or ointments.

These instruments are often of great service, especially in those cases in which the disease has just located itself in the deeper parts of the canal. But when the gleety discharge has existed for many months, and granulations have developed themselves, they are not so reliable; such cases are treated with more precision and certainty with the aid of the endoscope than by any other means. This instrument, which has



done so much to familiarize us with the pathology of the urethra, enables us to make our applications to the actual seat of the disease, through the endoscopic tube, by means of cotton wadding or fine sponge attached to a rod with screw ends. The great advantage of this treatment is, that as it is often necessary to resort to strong solutions (viz., arg. nitrat. grs. xx. to xxx.) repeated at intervals of from three to six days, it is confined entirely to the diseased spot. I am now keeping a record of a number of cases treated in this manner, and hope shortly to lay before you a full report of each case.

On page 409 is an outline of Desormeaux's endoscope, given for the benefit of those who may not have seen the instrument.

ART. V.—*Notes on the Physiology and Pathology of the Nervous System, with Reference to Clinical Medicine.* By MEREDITH CLYMER, M. D. Univ. Penn., F. C. P. Phil., etc., etc.

DISSEMINATED SCLEROSIS OF THE NERVOUS CENTRES.

(Continued from the May number, p. 262.)

CASE VI.—*Cerebro-Spinal Form, with Atrophy of Optic Disks of both Retinæ.*—(MAGNAN. Mémoires de la Société de Biologie, Paris, 1869.)

M. S., female, aged thirty-four; admitted July 6, 1869. At thirteen years of age (1848), had an attack of typhoid fever, which lasted six weeks. During convalescence the sight began to grow dim, and she soon became totally blind. The general health was good, and her intellect unimpaired, until the beginning of 1867, when trembling of the hands and arms was noticed whenever any movement requiring nicety or precision was attempted. The tremor gradually grew worse, and extended to the lower extremities. During the eight months previous to above date, walking was very difficult, she was confined to her chair or bed, and was unable to feed herself. Sensory troubles now appeared, pain-spells occurring in different parts of the body, particularly on the right side. While the patient remains sitting, and is perfectly quiet, there is nothing peculiar in her appearance; but, the moment she is spoken to, and she turns to answer, on moving her head, it is seized with an irregular, interrupted tremor; there is nystagmus; and the muscles of the trunk are agitated, as if by a series of shocks; the arms and hands too are tremulous, to a degree to prevent a glass of water being carried to the lips without spilling the contents. When standing, or on attempting to walk, the legs begin to tremble, and then sudden, irregular, spasmodic contractions of the muscles occur, which are quickly propagated to the muscles of the trunk, setting the whole body convulsively shaking. Speech is drawling, hesitating, and slightly thick. Darting pains and cramps are felt in the legs, sometimes in the arms, particularly of the right side; and the pains extend to the back. In the right cheek there is severe pain in the course of the branches of the facial nerve, and especially in the direction of the inferior dental and frontal branches. At times a disagreeable sensation of burning is felt in the cheek and legs, with tinglings along the spine. On several occasions there has been a feeling of heat in the belly, and, within a few days of date, a tympanitis was developed, which seems to be passing off.

The intellect is weakened, but there is no special delusion. Sight is entirely lost. Ophthalmoscopic examination shows: in the right eye, optic disk oval, and of a pearl-white tint, with the vessels very small; in the left, optic disk white, contour well defined, and both arteries and veins of less than natural size.

REMARKS.—The point of special interest in this case, reported by Dr. Magnan, is the probable occurrence of sclerosis of the optic nerves many years previously to the development of disseminated cerebro-spinal sclerosis, which was the diagnosis. There is likely tissue change in the facial nerve. When, Dr. Magnan remarks, similar facts are more numerous, and we have had larger opportunities of studying simultaneous alterations in both the peripheral and central districts on the one hand, and, on the other, extensions of the morbid processus from the centres, primarily affected, to the periphery, we shall be better able to understand the intimate relations between these several localizations.

The following is an abstract of Oppolzer's case, already referred to (page 5). Anatomically it is an example of multilocular sclerosis, while clinically it differs from all the other collected cases by the persistence of the trembling when the patient was at rest.

CASE VII.—*Spinal Form.*—(OPPOLZER. Canstatt's Jahresbericht, 1861, vol. iii., p. 78, from Spital-Zeit.)

Male, aged seventy-two, of diminutive stature, admitted to the clinic June 20, 1860, with violent trembling, which hindered him from using his hands. He never suffered from any serious illness until 1848 (aged sixty), when, during the bombardment of Vienna, he had a severe fright. He was carried to his home; and had scarcely recovered from the attack, when, a bomb bursting near the house, brought on another. A few hours afterward, on trying to take food, he found himself unable to use his hands, or, on attempting it, they began at once to tremble violently. After a while his lower limbs commenced to tremble, but less violently, and he could still walk. In spite of treatment, the disorder grew worse. The *trembling persisted even when he was at rest*, and involved other muscles. Paralysis (paraplegia?) came on. After a few years he was unable to stand erect, and as soon as he made the attempt, there was an irresistible tendency to fall forward, so that, to avoid toppling over, he was obliged to lay hold of any near object, or to walk hurriedly. His intellectual faculties and his senses had slowly but progressively diminished. Tea, coffee, and spirituous drinks, increased the trembling. The agitation of the lower limbs was more marked in the evening, and when the patient had walked during the day. Six months previously to admission, the sphincters became paralyzed. Five weeks before, after a severe attack of vertigo, he dropped down suddenly, and was unable to rise, but did not lose consciousness. Since that time emaciation had rapidly increased; the patient can stand and walk for a very short time only, and with very great effort.

Condition on Admission.—Great emaciation; earthy tint of skin, which is covered with numerous epithelial scales; perspiratory secretion increased on face, but seems lessened in other regions; temperature of skin lower than natural. Muscles of face, tongue, neck, and upper limbs are affected with violent trembling, which *never ceases during the waking state*, and is completely suspended only during profound sleep. The lower limbs shake periodically only, and when there is a general exacerbation of all the symptoms. The muscles, which are the seat of the trembling, are rigid at the same time, especially the muscles of the neck and shoulders. Pupils dilate and contract naturally. Mouth incompletely closed, and saliva dribbles out of both corners over chin. Articulation embarrassed. Sensibility everywhere normal; muscles contract, though somewhat feebly, by galvanic excitation. Slight dulness over apex of right lung, with diminution of respiratory murmur. Frequent vertigo; occasional cephalalgia; stools natural; urine alkaline, and contains some pus. Questions answered slowly, but pretty clearly. Physiognomy expressive of indifference and apathy.

June 22d to 24th; severe diarrhœa, and involuntary stools, which yielded to opiate injections.

June 25th; but little sleep last night, and delirium; about 10 A. M. had an epileptiform seizure, during which the head was drawn to the right, right eye turned outward and upward, and the left downward and inward. Eyelids and tongue at the same time continually oscillating; while the muscles of the face were rigid. Upper and lower extremities flaccid, offering but little resistance when moved. Complete loss of consciousness, pulse and respiration weak and irregular: duration of fit, eight minutes.

Between 1st and 7th of July, fresh seizures, after which the trembling ceased for half an hour, and then recurred with increased severity. General sensibility diminished from day to day; and the face had a besotted look, like that of typhoid fever. Abdomen swollen, stools involuntary;

the patient lay in a sort of sleep, and it was impossible to fix his attention, answering in monosyllables the questions put to him. Strength failed rapidly, pneumonia set in, and death July 11th.

Autopsy.—Tubercular cavities at apex of right lung, and granular hepatization of lower lobe. In the substance of the right optic thalamus, apoplectic cyst of the size of a small bean, the walls of which contained pigment. The *pons varolii* and *the medulla oblongata* manifestly *indurated.* The spinal cord was firm, and *the lateral columns*, principally in *the lumbar region, presented opaque gray striæ.* On a microscopical examination, there was found, in the substance of the pons varolii and of the medulla oblongata, an abnormal production of connective tissue. The opaque striæ in the lateral columns of the cord were due to the presence of connective tissue in process of development.

In the subjoined tables, sixteen cases of disseminated sclerosis are analyzed with reference to age, sex, anatomical characters, disorders of motility, sensibility, special senses, intellect, and the cause of death. They have been carefully selected from the whole number of cases with autopsy now on record, and may be considered as fair types of the several varieties of this lesion of the brain and spinal cord. Case VII. has points of special interest, and the diagnosis offered some difficulties. The paroxysmal lightning-pains in the lower limbs, the tight band around the waist, slight weakness of sight, impossibility of walking with the eyes shut, and the cutaneous anæsthesia, all pointed to locomotor ataxy. On the other hand, the sight-troubles were developed at a late stage, and were insignificant; muscular weakness in the lower limbs was an early, indeed the first symptom; and, although the patient could not walk in the dark or with his eyes closed, still the peculiar, wild thrusting out of the limbs so constant and proper to locomotor ataxy was wanting. There were, moreover, tremor, rigidity, and convulsive movements of the lower extremities, all of which are exceptional in the latter disorder. Finally, the notion of position was retained, and this very rarely happens in the advanced stages of locomotor ataxy. At the autopsy various sections of the spinal cord showed patches of sclerosis, irregularly scattered over all the columns, particularly in the cervical region, but it was on the posterior columns that they were found of greatest extent, thus accounting for some of the embarrassing symptoms, the lancinating pains, etc.

Analysis of Sixteen Cases of Disseminated Sclerosis.

CASES.	AGE.	SEX.	LESIONS.	DISORDERS OF				REMARKS.
				MOTILITY.	SENSIBILITY.	SP'L SENSES.	INTELLECT.	
I. Cruveilhier. Atlas d'Anatomie Pathologique, Liv. xxii., Pl. II., Fig. 4, p. 22.	37	Female.	Indurated spots on the anterior pyramids, right olivary body, and corpora restiformia. The roots of the hypoglossal, glossopharyngeal, and pneumogastric nerves, gray. Patches of induration on anterior face of cord, on the pons, inferior surface of cerebral peduncles, corpus callosum, and fornix.	At æt. 31, weakness felt in left leg; three months after, in right. Later, superior extremities affected; they are feeble and tremulous, but are still capable of use in taking food. Finally, total loss of motion of all the limbs. Articulation embarrassed; deglutition difficult. When spoken to, the muscles of the limbs and trunk are the seat of involuntary movements, which cause the whole body to tremble.	Natural.	Sight feeble.	Perfect.	Lungs tuberculous. Lobular pneumonia. Bronchitis. — Death from pulmonary disease.
II. Cruveilhier. Atlas d'Anatomie Pathologique, Liv. xxxviii., Pl. v., Fig. 1 and 1', pp. 1 and 2.	38	Female.	Gray degeneration of the cord in the form of patches, more or less large, and in much greater number on the posterior than anterior columns of the cord. No alteration of the nerve-roots. Similar patches on several points of the pons varolii. All have a certain depth.	Eighteen months previously, tinglings were felt in the soles of the feet and in the leg, and, almost concurrently, weakness in the lower limbs. Soon after, the arms began to tremble. Fifteen months subsequently, the patient dragged her legs, particularly the left; both constantly gave way under her. She cannot walk without assistance. The left leg more feeble than the right.	Little or no sensation in lower limbs; sensibility very much lessened in the upper. The inferior extremities have at no time been affected with cramps or convulsive movements.	—	—	Death from pleurisy.

III. Friedreich. Reported by Valentiner. Deutsche Klinik, No. 14, 1856	21	Male.	Patches of sclerosis on the mamillary tubercles, the cerebral peduncles, pons varolii, corpora olivaria, in the substance of the cerebral peduncles, and the medulla oblongata.	Unsteadiness of movements; staggering gait; violent trembling, happening when the patient is spoken to, and which affects the movements of the hands; similar tremor of the head; later, nystagmus.	Occasional attacks of vertigo; pains in the head, particularly in the occipital region. — Sometimes shooting pains in the legs.	—	At the beginning, mental excitement; toward the end, a kind of stupor.	Death from paralysis of the pneumogastric.
IV. Friedreich. Reported by Valentiner. Deutsche Klinik, 1856, No. 14.	20	Female.	Patches of sclerosis on the surface and in the interior of the pons varolii. Induration of the cerebral substance which surrounds the lateral ventricles. Patches of sclerosis on the cord.	Onset at æt. 17, after sudden chill. Weakness of right leg, afterward of left. Trembling of the hands when they are used, afterward participation of the muscles of the eye and tongue. Speech-trouble. Toward the end total paralysis of the lower limbs.	Sensibility of limbs slightly diminished.	—	At the same time, with difficult articulation, a notable diminution of intelligence.	Death from sloughs over sacrum.
V. Zenker. Zeitschrift fur rat. Med. Bd. xxiv., Hft. 2 and 3.	30	Female.	Induration of ependyma of both lateral ventricles. Sclerosis of the cornus ammoni, corpora striata, fornix, peduncles of the pineal gland, corpus callosum, tænia semicircularis, pons varolii, cerebral and cerebellar peduncles, and upper part of the spinal cord.	At æt. 26, weakness and trembling of lower limbs; later, of the hands and head. The unsteadiness of gait prevented any employment, although she could still use the hands. At æt. 30, tremor, not apparent when the patient was quiet, nor during sleep, seized the limb when any movement was attempted; on this, the upper and lower extremities, and the head, began to tremble; in the erect position the whole body began to shake. At the terminal period, complete paraplegia. Embarrassment in movements of tongue.	Violent cardialgic spells at the onset and during the course of the disease. Cutaneous sensibility abolished toward the terminal stage. Muscular sense lessened from the beginning.	Slight diminution of sight from the outset.	—	Death from sloughs.

Analysis of Sixteen Cases of Disseminated Sclerosis.

CASES.	AGE.	SEX.	LESIONS.	DISORDERS OF				REMARKS.
				MOTILITY.	SENSIBILITY.	SP'L SENSES.	INTELLECT.	
VI. Vulpian. Union Médicale, June 7, 1866.	51	Female.	Patches of grayish color, variable size, distinct, situated in one district on the right lateral columns of the cord, in others, on the left lateral columns; and, at certain points, on the anterior columns, and at others, on the posterior columns. Same coloring on the medulla oblongata, where it extends to the lateral portions and posterior surface, and reaches as far as the floor of the fourth ventricle. The lower half of the left olivary body has the same hue. The size of the cord is evidently diminished, and in those points where the gray tint is most extended, there is, at the same time, well-marked antero-posterior flattening.	At æt. 24, the left foot was suddenly twisted, and began to drag immediately afterward. Three years later, after a fall, the right leg becomes weak, then the right arm, and, finally, ten years later, the left arm. Twenty years after onset (1865) the condition of the patient was: permanent contraction of all the limbs; spells of spasmodic rigidity coming on very frequently in the muscles of the trunk and limbs; when one of the feet is flexed, and kept in that position, there is immediately severe trembling, which is very difficult to stop, and impossible at times when the right foot is the subject of experiment.	Slight hyperæsthesia.	—	—	Death from bronchitis.
VII. Charcot.	48	Female.	Patches of sclerosis in the walls of the lateral ventricles, in the substance of the corpora striata, on the surface, and in the substance of the pons, and in the posterior and lat-	At æt. 36, diminished power in the lower limbs, which become easily tired; at æt. 42, walking impossible without assistance. Later, rigidity of the inferior extremities, and, at times,	Two years before onset, darting pains in the limbs during the	Sight weak.	Good.	Death from sloughs.

			eral columns of the cord, particularly in the cervical region. A centre of softening, about the size of a walnut, corresponding to the posterior convolution of right parietal lobe.	convulsive tremors in them. Three weeks before death, apoplectiform attacks; the legs became rather flaccid—could not be raised beyond the bed. Four days later, second attack of left facial hemiplegia, followed by paralysis of the left arm.	night, once or twice a month; later, waist-pains. General sensibility somewhat lessened. Loss of notion of position of limbs. Headache, lasting two or three days each month.			
VIII. Vulpian.	53	Female.	Reddish-gray spots on the pons varolii, olivary bodies, particularly the left; on the corpora pyramidalia, especially the right; in the white substance of the centrum ovale. Patches scattered on the different columns of the cord to within two and a half centimetres of its termination, where the cord was entirely sound.	Invasion at æt. 34. After numerous periods of pauses, improvement, and aggravation, walking impossible at æt. 45. The patient could still stand, but when she did so, all the limbs began to tremble. Severe tremor in the feet when they are forcibly flexed, or when an attempt is made to keep them so. Impossible to flex the thigh on the pelvis, the leg on the thigh, the foot on the leg, on part of patient. Although tickling sole is felt, no reflex movement. The arms began to grow weak at æt. 52; no other special symptom in them.	Lancinating pains in the lower extremities, especially the left; sensibility normal on both sides, or perhaps slightly increased.	—	—	Death from sloughs.

Analysis of Sixteen Cases of Disseminated Sclerosis.

CASES.	AGE.	SEX.	LESIONS.	DISORDERS OF				REMARKS.
				MOTILITY.	SENSIBILITY.	SP'L SENSES.	INTELLECT.	
IX. Charcot. Reported by Bourneville, p. 30.	41	Female.	Patches of sclerosis in corpora striata, on external wall of right lateral ventricle, right optic thalami; in pons, and nearly whole extent of cervical enlargement of cord, except posterior columns, and lateral, anterior, and posterior columns of dorsal region.	At æt. 28, fall during pregnancy; two years after, weakness of legs after long walks; two years more, could not walk without help. Amelioration, lasting six months. Progress resumed, and at æt. 35 little use of lower limbs, which were also stiff. At æt. 39 both upper extremities affected, left more than right. An attempt to carry any thing to mouth caused trembling of right side, and slight tremor of head.	Cutaneous sensibility natural. Feeling of heaviness in lower limbs. Darting pains about fifth dorsal vertebra, below right breast, and around waist.	Sight weak, particularly of right eye; diplopia.	Good.	Death from pneumonia and sacral sloughs.
X. Charcot. Reported by Bourneville, p. 44.	36	Female.	Patches of sclerosis in optic thalami, lateral ventricles, pons varolii, cerebellum, peduncles of cerebellum, left olivary body, and cord; the nerve-cells of anterior cornua are shrunken; atrophy and sclerosis of optic nerves.	At æt. 30, after sight-troubles gait began to be unsteady; there was slight hold of the ground. Paraplegia progressive until total; rigidity of lower extremities; upper extremities become weak; tremor on any voluntary movement.	At outset pains about shoulders and in temples—then numbness of feet—seemed to walk on down. Contactile sensibility lost about insteps and lower third of leg. Analgesia of both lower limbs.	First feeling of bar of wood in eyes; a little while after, dimness of sight.	Good.	Death from pneumonia.

XI. J. C. Morris and Weir Mitchell. Transactions of the College of Physicians of Philadelphia, 1868. American Journal of the Medical Sciences, July, 1868.	62	Male.	Patches of sclerosis on anterior and lateral columns of spinal cord.	At æt. 37, weakness in left lower limb, which, in a few years, extends to right; paraplegia progressive until complete; left upper extremity affected, then right. Patient condemned to perfect immobility; unable to feed himself.	At outset sense of weight, with numbness in left leg; also sensation of a bar, an inch or so wide, around left leg; then in right. Sensibility in its several forms scarcely altered.	—	Perfect.	Tubercles in lungs and intestines.
XII. Ludwig Leo. Deutsche Archiv. für Klinik Medicin, 1868, p. 151.	34	Male.	Patches of sclerosis on walls of lateral ventricles, fornix, cerebral peduncles, ependyma of fourth ventricle, and corpus callosum. Optic nerves sclerotic to chiasma; corpora restiformia; calamus scriptorius; pons varolii. Sclerosis of posterior, anterior, and lateral columns of cord.	At æt. 20, headaches and vertigo until æt. 26, when a right hemiplegic attack; improvement; progressive unilateral paresis; at æt. 28, gait so unsteady that he cannot walk without aid. Another attack, left side; paraplegia progressive; gait tottering; when lying down can extend and flex lower limbs, but the motions brisker than natural; exaggerated reflex movements. Upper extremities but little affected; tongue embarrassed; series of apoplectic attacks; paralysis complete; tremor of hands and tongue; muscular contractions.	Cutaneous sensibility in its several forms lessened—not able to localize sensations felt. Pains along spine, in forehead, and in the legs, extending to the toes.	Diplopia at outset; myopia.	Perfect.	Exhaustion.

Analysis of Sixteen Cases of Disseminated Sclerosis.

CASES.	AGE.	SEX.	LESIONS.	DISORDERS OF				REMARKS.
				MOTILITY.	SENSIBILITY.	SP'L SENSES.	INTELLECT.	
XIII. Charcot. Vulpian, Union Médicale, 1866.	46	Female.	Patches of sclerosis on anterior and antero-lateral columns, with lineal streaks on posterior columns in dorsal region.	At æt. 45, weakness of legs; rapid paralysis of all the limbs and of the trunk; sits up with difficulty, and can scarcely support head when raised from pillow; contraction of lower extremities and left arm. Urinary and fecal incontinence.	Three years before motor-troubles, darting pains through thighs and legs. Sensibility preserved in paralyzed limbs.	—	—	Death from exhaustion, four years from onset. Probably previous syphilitic troubles.
XIV. Charcot. Vulpian, Union Médicale, 1866.	43	Female.	Patches ot sclerosis on pons varolii, left anterior corpus pyramidalis; very large patch on right lateral column of cervical region of cord. The motor oculi externus nerve altered.	At æt. 38, feebleness of lower limbs; a year later, vertigo, apoplectic attack, followed by right hemiplegia; three years after, a second attack, followed by contraction of flexor muscles of fingers, and forearm of right side; two years later, a third attack; right upper extremity wholly paralyzed and rigid; paraplegia and permanent extension of lower extremities.	Long subject to flying pains and facial neuralgia. After disease is established, pains in left ham and heel; feeling of heaviness and numbness in lower limbs.	Incomplete paralysis of left motor-oculi externus muscle, with internal strabismus and diplopia. Towards end darting pains in head.	--	Gradual sinking after last apoplectic attack, December, 1861; death, February 7, 1862.

XV. Charcot. Ordenstein, p. 78, and Bourneville, p. 129. (Case reported by Ordenstein; autopsy by Bourneville.)	30	Female.	A small patch of sclerosis eleven centimetres above olivary bodies; a larger on cervical enlargement; two, one on right, other on left posterior column; in brachial enlargement, just outside the origin of sensory-nerve roots. A large patch surrounding aqueduct of Sylvius, with processes extending into pons; several of considerable size on the walls of lateral ventricle, which are lost in the substance of the hemispheres; in cerebellum, a small patch on right side, near the gray matter of the surface; another in pons.	Unsteadiness of gait, amounting to titubation; difficulty of speech; tremor on any voluntary motion, particularly in left arm; cannot carry left hand to head or mouth directly; tremor of tongue; muscular force of limbs fair; tendency to fall backward in walking; no permanent rigidity; no spasmodic muscular contractions.	Cutaneous sensibility slightly increased; darting pains in head. Onset with spells of vertigo, and soon after hysterical globus in throat.	Nystagmus, in right optic disk, beginning of atrophy; in left, slight dilatation of veins; sight a little weak, but variable.	Fair; very emotional.	Tubercles in lungs. Death from phthisis.
XVI. Skoda. Reported by Barwinkel. Wien Med. Halle, iii., 13, 1862. Schmidt's Jahrbuch, No. 119, p. 294; Year-Book of New Sydenham Society, 1863, p. 100.	34	Female.	Walls of ventricles, fornix, pons varolii, medulla oblongata, and spinal cord remarkably indurated. Optic nerves hard and flattened. In some opaque, reddish spots of the brain, the nerve-elements are destroyed by new connective tissue; in the pons and cord there was proliferation of connective tissue, and obliteration of the vessels.* Fatty degeneration of muscles.	At æt. 32, feebleness of lower extremities, and soon after tremor of right hand, and then of left—occurring only on voluntary movements. Both progressive. Speech somewhat indistinct.	Began with vertigo, and pains in head and sacrum. Cutaneous sensibility slightly lessened.	Good.	Good.	Death from small-pox.

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* In this case there would appear to have been not alone disseminated sclerosis, but also diffuse sclerosis.—M. C.

B. CORTICAL SCLEROSIS OF THE BRAIN AND SPINAL CORD.

The *cortical* or *annular* form of cerebro-spinal sclerosis has recently been described by Dr. Vulpian, and an illustrative case published by him, a brief of which will be sufficient to show the symptoms and nature of the morbid process.

L. B. was admitted into la Salpêtrière, November 7, 1861, with an affection of the spinal cord which began when she was fifty-two years of age, by weakness in the lower extremities, and was apparently neither preceded nor accompanied by lightning-pains nor sight-troubles. This paresis of the lower limbs gradually, but very slowly, increased, and fourteen years after the onset the patient could only walk with crutches, or when helped by some one. Eighteen months later she could scarcely maintain the erect position, even when leaning on a support. Cutaneous sensibility was, when she was examined in 1862 and 1868, very much lessened throughout the lower extremities; but, in 1868, on the dorsum of the feet it had reappeared, and was nearly natural.

There were none of the ordinary symptoms of sclerosis of the posterior columns; there was but little, if any, ataxy of movement; in walking, the left foot was thrown forward in a somewhat exaggerated manner, but there was really no true motorial incoördination. Lying in bed, she could raise one or both legs without any involuntary deviation, and keep them in that position without their trembling. The muscular force of the lower limbs, tested in the horizontal position, appeared good, except perhaps during the last three months.

In 1866, for the first time, lancinating pains were felt in the lower extremities, accompanied by convulsive jerks; but these pains did not seem to have the distinctive lightning (fulgurant) character of locomotor ataxy. Notion of position of the limbs was perfectly good, and, except some weakness of vision, there were no sight-troubles.

Toward the end of 1867, general tremor of the whole body was noticed, whenever the patient attempted to stand up, or tried to walk.

The symptoms collectively were still evidently very differ ent from those of posterior sclerosis of the cord, and it was recognized that the lesion of the cord, whatever it might be, must, in some respects at least, be other than that of locomotor ataxy. This probability was verified by the autopsy. Instead of fascicular sclerosis of the posterior columns, the cortical layer of the white substance was found sclerotic throughout the periphery and in the whole length of the spinal cord. There was also spinal meningitis, particularly well marked on the posterior face of the cord, but easily recognizable on the anterior and lateral faces.

Proceedings of Societies.

MEDICAL SOCIETY OF THE COUNTY OF NEW YORK.

Adjourned Stated Meeting, March 21, 1870.

DR. GEORGE T. ELLIOT, President, in the chair.

REPRODUCTION OF BONE.

DR. WILLIAM R. WHITEHEAD read a paper upon this subject, which is published in the present number of the JOURNAL.

DR. J. C. NOTT said that, some thirty years ago, Dr. Toner, now of California, then a young man commencing practice, had published several cases of onychia, in which he had removed the phalanx, leaving the periosteum, and obtained a reproduction of the bone. Impressed by these cases, the speaker had himself followed this practice ever since, and with success. He had heard that old Dr. Dudley was accustomed to treat onychia in the same manner. In a case of necrosis of the upper jaw he had, in removing the bone, left its periosteum *in situ*, and the bone was regenerated.

DR. SAYRE had from his earliest practice removed phalanges in the manner above described, which he had been taught by Dr. Dudley. It was very important, in such cases, to dress the finger, during the process of healing, so as to preserve its normal shape; otherwise the nail would have become hooked over, or some other deformity result. One of the speaker's sons had smashed his finger throughout its whole length, so that at first amputation seemed unavoidable. But by careful preservation of all the periosteum almost complete reproduction of all the phalanges was secured, and the finger was now serviceable, though somewhat shorter than its fellows. The doctor had resected subperiosteally four and one-half inches of the femur of a young man. The bone was completely reproduced, the limb equal in length to the other; and the lad had last winter won a prize in a skating-match.

Dr. Chadsey related a case of necrosis of the entire tibia, which was removed as a sequestrum; new bone having formed around it firm and serviceable, though at first large and misshapen. It gradually became reduced to about the natural size. A patient of his had been kicked by a horse above the eyebrow, severely comminuting the outer table. In removing the pieces of bone, the periosteum was left attached to the skin; and the lost bone was restored with no depression or deformity.

Dr. Bibbins referred to two cases of necrosis of the lower jaw, from gangrenous stomatitis in children, which had come under his care at the Nursery Hospital, Randall's Island, in 1850. In one of them, he had resected the bone from the canine tooth to the angle of the same side; in the other, from the canine tooth to the angle of the opposite side. The periosteum was carefully left in both cases, and in both the part removed was reproduced, though of course without teeth or alveolar process.

The President said that in some cases of pelvic deformity which would otherwise necessitate the Cæsarean section, it had been proposed to resect portions of the pelvic bones so as to permit of delivery, trusting to the undisturbed periosteum to restore the loss. Osteophytes, when they occurred in the pelvis, were most apt to be seated along the linea iliopectinea, and might be a cause of rupture of the uterus during labor. He referred, also, to the cases of incurable deafness produced by the sealing up of the external auditory canal by a bony growth.

The Society adjourned.

Bibliographical and Literary Notes.

ART. I.—*The Physiology of Man. Secretion, Nutrition, Movements.* By AUSTIN FLINT, JR., M. D. Vol. iii. New York: D. Appleton & Co.

THE reader of this last volume of Dr. Flint's treatise upon Physiology is not long in ascertaining that he is dealing with thoroughly honest work. This is no small eulogy in these days of slipshod book-making. This first impression is further strengthened by a careful examination. Any one who desires to test how thoroughly the work represents the existing state of physiological science has only to select any single chapter, and examine for himself the latest authorities upon the subject treated. He will be surprised to find how little there is to add to Dr. Flint's comprehensive review. Every thing of importance will be found to have been studied, sifted, and the chaff separated from the wheat by a vigorous mind well trained in physiological investigations. The volume is an improvement upon its predecessors. The training acquired by past labors is not without its legitimate results, as exhibited in improvement of style, and the general management of the various questions that come under examination. Moreover, the fruits of German scholarship have been more freely laid under contribution than heretofore, and this we regard as adding very greatly to the value of the subject-matter.

The three principal divisions of the present volume treat respectively of Secretion, Nutrition, and Movements—subjects that hitherto have hardly received fair consideration in systematic works upon physiology written in the English language.

Under the head of Secretion we find a preliminary chapter in which all the general principles influencing secretion are clearly stated. Then follows a more particular account of the various fluids belonging to this category, together with descriptions of the physiological structure of the organs engaged in their manufacture or elimination, a work in which Dr. Flint especially excels. Particular mention may be made of the article on milk; the description of the modern view of the anatomy of the kidney, which certainly will be new to many; the temperate, judicial manner in which recent experiments upon excretion are criticised; the account of the physiological anatomy of the liver, concerning which the microscope has revealed a good deal to modify our old notions, and the study of the functions of the liver, a subject with which the name of Dr. Flint is especially identified, and in connection with which we find much of value in the way of original observation. The ductless glands are briefly and well described, but the subject is a thankless one.

The chapter on Nutrition will be found of general interest to the non-professional reader. The whole subject of movements will be new to the majority of students. Dr. Flint has placed them under obligation of no slight order in a clear and brief statement of the recent investigations of Helmholtz, Dubois-Reymond, Aeby, Marey, and others, upon the changes that take place in muscle during contraction, and in the description of various tissues by which locomotion is effected.

The work closes with a good account of the mechanism of voice and speech, which it would be well for those engaged in training the organs involved, in spite of unavoidable technicalities, to read and study carefully.

Dr. Flint is tolerably tenacious of his opinions, and is therefore open on many points to the criticisms of those who are differently minded; but no one can complain that a fair hearing has not been given to differences of opinion, so that independent judgment is not interfered with. When the entire work is completed, and we believe there are two volumes still to come, it is destined to fill the place in the English language of that of Longet in the French, and a number of standard works on the same subject in the German tongue. Perhaps the highest praise, with which we can conclude, is to predict that it will be acknowledged to occupy the place so long and ably filled by the now rather antiquated "Physiological Anatomy and Physiology of Man," of Drs. Todd and Bowman.

Art. II.—*The Cell-Doctrine: its History and Present State. For the Use of Students in Medicine and Dentistry. Also a Copious Bibliography of the Subject.* By James Tyson, M. D., Lecturer on Microscopy in the University of Pennsylvania, etc. With a Colored Plate and other Illustrations. Philadelphia: Lindsay & Blakiston, 1870. 12mo, pp. 150.

As of fundamental importance to the study of physiology and pathology, the very *grundlage*, as our German friends would say, of these sciences, Dr. James Tyson, in a nicely-printed little volume, attempts to give a continuous history of the evolution of the "cell-doctrine" up to present state. He has done so intelligently, thoroughly, and briefly; and that, not only as a well-informed bibliographer, but as a competent microscopist practically acquainted with the subject, and qualified to express opinions respecting conflicting views. While he has fairly stated the doctrines of all the leading histologists, he adopts, with some modifications, those of Dr. Beale. There are a few points of difference, "some, perhaps, purely in mode of expression, but others as to matter of fact." Dr. Tyson believes that the central matter of the cell—the "germinal matter" of Beale—is not in all instances structureless, but is sometimes granular, as Robin holds, and as Beale himself once taught. Nor would he speak of "formed material" as invariably "dead," where it is the seat of so many important vital endowments, as in muscle and nerve. "In some situations, it is indeed lifeless, as when it becomes the secretion of glands, as bile and milk, or the peripheral part of epithelial cells. It simply is devoid of a power of multiplying or growing by itself, depending for its increase on the conversion of the germinal matter."—p. 111. Hence, the term of "non-germinal" or "non-germinating" matter is suggested, since this is the only attribute common to all formed material. In morbid processes the germinal matter is active, being increased, diminished, or perverted. Dr. Tyson thinks that the observations of Beale reconcile the discordant views with regard to the so-called *exudations*, for we need not suppose an excessive dislocation of structure to admit the passage of large cells, or be compelled to restrict the origin of those cells to points outside the vessels.

The reader of this book will find collected, in a ready form for study and reference, all the essential matter, on an interesting and important question, scattered throughout many expensive works.

The fact that a third American edition of this little work[1] has been called for, sufficiently attests its popularity. Though it belongs to the class of handy books, to which every well-constituted mind ought to feel a natural repugnance, it contains so much useful information that an exception may properly be made in its favor. Its purpose appears to be not so much a help to the lazy as to put in a convenient form such material connected with the clinical study of disease as may best aid the busy man in his daily experience. Both the student and physician have a right to like the book, and would do well to consult it freely.

This little work[2] contains a good deal of information useful alike to the country practitioner in regions where the absence of the dentist compels an assumption of responsibility, and to the city physician, who, if not called upon to directly interfere, is still constantly consulted by mothers with regard to the special class of disfigurements to which irregularities of the teeth give rise.

The speedy appearance of the third edition of Dr. Williams's Manual[3] is an evidence of the esteem in which the book has been held by the profession. It was not that there were not many and good works treating of this subject, that this volume, small in size, but solid in substance, took such rank as has been accorded it. It was simply because it brought within reasonable compass and made plain, even to those not versed in ophthalmological science, whatever there was worth knowing in this important branch of medicine. And, by reason of this condensation and this simplicity, the book is admirably adapted for the wants of the general practitioner. Indeed, we know of no other of equal size that will bear comparison with it. So short a time has elapsed since the appearance of the previous edition, that no alterations have been found necessary. The mechanical execution of the volume deserves notice and commendation, and many of our publishers might take a lesson in book-making from the superb work in the volume before us.

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[1] A Manual of Clinical Medicine and Physical Diagnosis. By Thomas Hawkes Tanner, M. D. Third American edition, from second English edition. Revised and enlarged, by Tilbury Fox, M. D. Philadelphia: Henry C. Lea. 1870.

[2] Irregularities and Diseases of the Teeth. By Henry Sewell. John Churchill & Sons. 1870.

[3] A Practical Guide to the Study of Diseases of the Eye: their Medical and Surgical Treatment. By Henry W. Williams, M. D. Third edition, revised and enlarged. Boston: Fields, Osgood & Co. 1869. 12mo, pp. 422.

THE application of photography,[1] for securing exact representations of pathological conditions, marks a distinct advance in descriptive or rather delineative medicine, and Dr. Damon, taking advantage of this process, has here given us an admirable collection of views of some of the more important skin-diseases, especial attention being paid to the various syphilitic eruptions. Mr. Balmano Squire, of London, and Hardy, of Paris, had previously published photographs of skin-diseases, but their plates were subsequently colored. In the set before us, the photographs are uncolored, and in some respects we deem this an advantage, for we thus secure absolute accuracy in the essentials of outline, form and elevation, which are perhaps more important in the make-up of skin-diseases than the changeable element of color. Mechanically these plates are very skilfully executed, and on examination with a lens they show the excellence of their work. The collection, as it now stands, is a most valuable addition to the department of medicine to which it relates, and should be in the hands of every one who practises this specialty.

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[1] Photographs of Skin-Diseases taken from Life. Under the Superintendence of Howard Damon, M. D. Boston: James Campbell. 1870. Twenty four plates, quarto, with descriptive text, bound in cloth portfolio.

The appearance of the third edition of this work[1] only confirms the high estimate we have previously, and on two separate occasions (*see* Journal, March and July, 1868), put upon it, and we are thereby absolved from more than a mere notice of this new edition. The book seems to have received a higher appreciation in America than in the home of its production, where, as the author tells us in his preface, which comprises just six and a half lines, the third edition is not yet required, and therefore he sends the MSS. here for publication. Since the appearance of the first edition, upward of ten dozen cases have been added, and the comments upon them have been in part rewritten and rearranged, and the book may now be considered to have attained full growth. We need only renew our previous commendations of the work, assuring our readers that it is a most sensible and practical manual for their guide in the treatment of the various derangements of digestion.

A new journal has appeared in London, called *The Food Journal; a Review of Social and Sanitary Economy, and Monthly Record of Food and Public Health.* Among the contributors are Drs. Carpenter, Beddoes, and Letheby, Profs. Gamgee, Hooker, and others.

A new edition of "Paget's Surgical Pathology," revised and reëdited by the author and Prof. Turner of the University of Edinburgh, is announced as nearly ready by the Longmans of London. The same house also promise the long-looked-for revisal of Watson's Practice.

Books and Pamphlets Received.—Proceedings of the Homœopathic Medical Society of Ohio. Fifth Annual Session. Convened at Cleveland, February 16 and 17, 1869. Pamphlet, pp. 112.

University of Maryland. Sixty-third Annual Circular of the School of Medicine. Session 1870–'71.

Sixteenth Annual Report of the Howard Hospital and Infirmary for Incurables. Philadelphia, Pa., 1870.

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[1] The Indigestions, or Diseases of the Digestive Organs functionally treated. By Thomas King Chambers, Honorary Physician to H. R. H. the Prince of Wales, etc. Third American edition, revised. Philadelphia: Henry C. Lea. 1870. 8vo, pp. 383.

Twenty-first Annual Announcement of the Woman's Medical College of Pennsylvania, 1870–'71.

Ninth Annual Report of the Board of Managers of the Woman's Hospital of Philadelphia, 1870.

Pepsin, its Physiological and Therapeutical Actions. By J. S. Hawley, M. D., Greenpoint, N. Y. Pamphlet, pp. 20. (From the Author.)

Reports on the Progress of Medicine.

PHYSIOLOGY.

By William T. Lusk, M. D.,
PROFESSOR OF PHYSIOLOGY, LONG ISLAND COLLEGE HOSPITAL.

1. *The Peptone Theory.*—Brücke, in a paper before the Academy of Sciences, in Vienna (*vide* Revue des Cours Scientifique, No. 50, 1869), expresses his dissent from the commonly-received opinion, that albuminoid substances necessarily undergo certain important changes before they become absorbed during digestion. The idea has its origin in the belief that diffusion and filtration take place in the peptones with much greater facility than in the albuminoids. Albuminoid substances pass with difficulty porous filters, because of their large-sized molecules. But they are not all alike in this regard. The white of an egg passes through a cloth only as a consequence of strong pressure; whereas the speed with which the albumen of Wurtz passes through a filter proves that difficult filtration makes no part of the integral properties of natural albumen.

It is claimed that albuminoid substances must be transformed into peptones previous to absorption; and by peptones are understood modifications of albumen, which neither coagulate spontaneously, nor by heat, nor upon the addition of acids. This proposition is essentially erroneous. In animals killed during absorption, and kept twenty-four to forty-eight hours in a cold spot until the muscles were dead, Brücke found on opening the abdomen that the chyle was coagulated in the lacteals. This coagulation was most likely due to the presence of an acid, as the experiments were mostly made upon mammalia fed with milk, and the contents of the small intestine were likewise found in each case both acid and coagulated. The coagulation was not due to the admixture of lymph, as it extended to the ampullæ of Lieberkühn, from which upon sections the chyle could be pressed out in the form of filaments, visible under the microscope. Here the quantity of blood-plasma, escaped from the capillaries of the villi, must have been small, and it is known that albumen in blood-serum is only precipitated in minute quantities on the addition of a very dilute acid. The coagulation can hardly have been due to fibrin, for the chyle, taken from the lacteals of the mesentery, and mingled with all the intestinal lymph, possesses but feeble coagulating powers. There is no doubt that the small intestine is capable of absorbing an albuminoid substance which does not possess the properties of the peptones. Aside from the albumen of the pancreatic juice, there is no difficulty in deriving from the nitrogenized aliments a coagulable albuminoid matter capable of absorption. This we do by taking the liquid obtained by the digestion of meat before those modifications are produced, which result from the prolonged action of the gastric juice, neutralizing it until it is only feebly acid, when an albuminoid matter is precipitated, which again is soluble in an excess of alkali, and can afterward be reprecipitated from the alkaline fluid by rendering it once more feebly acid. This precipitate, which really forms when the bile flows into the duodenum, has furnished Meissner with his parapeptone theory. It may dissolve anew in the contents of the small intestine. The albuminoid substance thus precipitated and redissolved may become modified by the pancreatic juice; or it may be absorbed either wholly or in part before it has lost the property of separating upon the commencement of an acid reaction. If this reaction occurs after death, we would have at the same time coagulation of the chyle in the lacteals.

But can albumen coagulable by heat be absorbed? The pancreatic juice contains an albuminoid body coagulable by heat, to which is attributed the *rôle* of emulsifying the fats. Fat is not only absorbed, but passes into the villi, carried along by the current of the liquid holding it in suspension. Now, we can hardly admit that passages large enough to permit the transit of fat-globules are too small to allow that of the molecules of the albumen, which has served for their emulsion, and if the passages allow the coagulable albumen of the pancreatic juice to traverse them, why not the albumen derived from food?

Bauer kept a dog fasting until the excretion of urea had become nearly constant. He then injected solutions of albumen into the large intestine, using sometimes peptones, sometimes the expressed acid muscular juice, sometimes albumen in a solution of salt, and finally albumen that had first been whipped, and then allowed to become fluid. In all of the three first instances, the large increase in the amount of urea excreted, proved that absorption had taken place.

Now, what is the state in which the albumen derived from food arrives in the intestines? Raw meat hashed, mixed with a good digestive fluid until the greater part has been dissolved, then filtered and neutralized until the precipitate, termed by Meissner parapeptone, is formed, furnishes a fluid after further filtration, presenting a feebly acid reaction, which passes readily through a filter, and which coagulates with heat. This soluble albumen is found during artificial digestion for more than four hours, even when kept at the temperature of the body at which the peptonizing process is most active. Brücke says it is not the pepsine, but the acid, that takes from albumen its coagulability. To restore the latter, we need therefore to completely neutralize the fluid. Thus albumen remains soluble long enough to pass in that state into the intestine. A dog kept fasting for forty-eight hours, and then fed on raw meat, was killed two hours after the reception of food. The stomach contained a notable quantity of meat in fragments, and a liquid, which, after the addition of carbonate of soda, and the production of the precipitate of the neutralization, coagulated upon application of heat. The intestine contained a mucous liquid, rich in SOLUBLE albumen, and the microscope showed the presence of muscular fibres. Absorption was going on actively, and the lacteals were filled with chyle. Another modification of albumen was sometimes found in the stomach, distinguished from the foregoing, by the property of forming a precipitate of a gelatinous appearance, in adding to the neutral solution a weak solution of acetic acid. In this condition it coagulated by heat, a property not possessed when the solution presented a neutral reaction. This modification was probably in part due to the presence of an increased quantity of phosphatic salts.

After the ingestion of cooked albumen, however, the products of digestion, which have been operated upon by the gastric juice, furnish a precipitate when neutralized, but the filtered fluid no longer coagulates by heat. As man scarcely employs any thing but cooked food, it would seem that the soluble albumen of the system is regenerated either by the precipitable albumen, or by some form of albumen so modified as not to coagulate by heat. But Kühne has found that the cooked fibrin of the blood is so modified by the pancreatic juice as to dissolve in a solution of common salt and form a liquid coagulable by heat; so it is probable that the pancreatic juice forms albumen coagulable by heat from the cooked albuminoid substances which pass undigested into the small intestine, and perhaps even from the precipitated albuminoid matters, which are found in the mass of the products of digestion after the action of the bile and the neutralization of the acid they contain. In the small intestine there is a notable quantity of soluble albumen, coagulable by heat, even when cooked meat has been used, which, however, proves nothing, as it may be derived from the pancreatic juice. Brücke obtained chyle from the lacteals by suction through a glass tube, and found that, when the contents were brought rapidly to the boiling-point in a tube placed in an oil-bath, they coagulated. But this may have been due, possibly, either to pancreatic juice, or to the lymph contained in the lacteals. This much, however, is certain, that there takes place an absorption of albuminoids which do not possess the properties of the peptones.

Brücke found it required fifteen hours to convert parapeptones into peptones at a temperature of 38° Celsius. Meissner says he never knew the transformation effected in so short time. Mulder found that it required thirty-two hours, at a temperature of 40° Celsius, to completely change albuminoids into peptones. In the female, with the intestinal fistula observed by Busch, aliments began to pass out of the fistula at the end of from fifteen to thirty minutes, and digestion was completed, after a full repast, in three or four hours. In so short a time the conversion of albuminoids into peptones must have been relatively incomplete. It is well to notice, too, that, after the patient had been strengthened by artificial alimentation through the lower part of the intestine, she was successfully nourished by the natural passages, notwithstanding the limited extent of the absorbent surface.

Admitting that the pancreatic juice continues the conversion into peptones, long before the albuminoid substances have had time to become materially modified, so soon in fact as the chyme reaches the small intestine, absorption begins; and if this portion of the intestine contains albuminoid substances in a state susceptible of absorption, they must necessarily be absorbed.

The question may be asked whether such albuminoid matters are really utilized by the economy—whether they do not disappear, while the albumen of the organism is regenerated by means of the peptones. When Busch first received his female patient, she was in a most deplorable condition. Believing that, so long as food was introduced by the natural passages only, she would not regain her strength, because of the shortness of the absorbent surface traversed, he introduced nitrogenized aliments by the fistula, so as to pass over the second portion of the intestinal canal. As this portion received neither gastric, nor pancreatic juice, were we to admit that all the albuminoid substances utilized after absorption consisted of peptones, we would have to presume a peptonizing activity on the part of the intestinal juice, in no wise warranted by our experience regarding it.

If albuminoid substances are exposed sufficiently long to the action of a digestive fluid, they become, according to Kühne, converted into leucine and tyrosine, yet no one would think of these substances as serving to regenerate the albuminoids of the economy. Yet in a series of metamorphoses why reject the first and last term, and fix upon the mean term, viz., the peptones, as the sole regenerators. Simply, because they were thought to rank highest in the scale of the substances allied to albuminoid matters, which were capable of absorption; but, now that we recognize that other albuminoid substances, less modified in character, are capable of absorption, we must give to these the precedence over the peptones. There is no longer any reason for regarding the regeneration of the albuminoids as taking place at the expense of products of decomposition.

The question for us to solve is simply this. A series of albuminoid bodies and their products are absorbed during digestion. Which of these substances serve specially to regenerate the blood, the muscles, and the nerves? The limit ought to be fixed, beyond which the albuminoid substances cannot further be modified in the intestinal canal, and still preserve the property of regenerating the albuminoid substances which fulfil the *rôle* of functioning constituent elements of the organism. The solution is attended with labor and difficulty perhaps, but seems by no means impossible, through the agency of direct experiment upon animals provided with intestinal fistulæ.

2. *Absorption of Fats.*—Dr. Th. Eimer (Virch. Arch., vol. xlviii., p. 1) has made some interesting experiments upon the absorption of fat, by the intestinal mucous membrane, which apparently demonstrate a direct system of canals between the cylindrical epithelium of the intestines and the subjacent lacteals and capillary blood-vessels. By means of fine sections through the intestines of frogs and small animals, previously hardened in a solution of osmic acid, Eimer succeeded in making out the presence of extremely delicate thread-like prolongations, proceeding from the lower extremities of the cylindrical cells of the intestines, and found that these could be traced far enough to show their direct communication with the thread-like processes of the underlying connective-tissue corpuscles. After introducing oil into their stomachs, the animals experimented upon were killed at different periods of digestion, and according to the stage of digestion the cylindrical cells were found either fully filled with fat-globules, or filled only in the upper or lower portions. By employing high magnifying powers (Hartnack's immersion system No. 10), it became possible to follow the fat-globules for a considerable distance into the prolongations, from the cells of the intestines. They (the fat-globules) were found to grow finer and finer, and were ranged in single rows like strings of pearls. By the addition of sufficient ammonia to partially neutralize the osmic acid, preparations were obtained showing that not only the connective tissue of the villi, but that of the submucous and muscular layer, furnish a canal system of the finest character, and form an immediate connection with the epithelium of the intestines filled with fatty particles.

This canal system opens, by trumpet-shaped orifices, into the central chylous vessel of the villi, and into the lymphatics of the mucous membrane, the submucous membrane, and the muscular layer. Certain of the processes terminate in the blood-vessels, and are in like manner filled with fat, explaining the relative abundance of fat in the portal vessels during digestion. Eimer found that an absorption took place in precisely the same way in the large intestines of frogs, after considerable quantities of oil had been introduced into the stomach.

Eimer found that the membrane upon the free surfaces of the cylindrical cells of the villi, described by Kölliker, was soft, and partially disappeared during digestion. The firmer portion which remained he regarded rather as belonging to the investing membrane of the cell, and appears during digestion to undergo a softening process, which allows us to admit the possibility of the passage of fat-particles through it, without imagining the existence of special pores or ducts.

3. *Glycogenic Function of the Liver.*—Dr. Austin Flint, Jr. (Glycogenic Function of the Liver. NEW YORK MEDICAL JOURNAL, January, 1869), removed in a number of cases a portion of the liver from a living animal, and, having plunged it into boiling water to arrest the fermentation of its glycogenic matter, demonstrated in it the absence of sugar. Then killing the dog, and rapidly tying the portal vein, and the vena cava above and below the hepatic veins, demonstrated in blood from the latter vessels an abundant presence of sugar. Whence he concluded that during life the glycogenic matter is undergoing continual change into sugar, but is not found in the liver-tissue proper, because it is washed out by the blood as soon as it is formed.

Pavy, in the September number of the same Journal, replies that the delay in applying ligatures to the vessels after destroying life is sufficient for a *post-mortem* production of sugar to take place, and that the sugar found in the hepatic veins by Dr. Flint was, consequently, the product of *post-mortem* fermentation. (It can, however, be conclusively demonstrated that the quantity of sugar taken from the right side of the heart, though in itself trifling, is really much larger than what is found in vessels of the body more remote from the liver.—*W. T. L.*)

4. *The Physiology of the Pancreatic Secretion.*—(N. O. Bernstein. Central Blatt., No. xlv., 1869.) Bernstein established permanent pancreatic fistulæ in animals by introducing into the pancreatic duct a lead wire, the two extremities of which were inserted into the opening while the central portion was so twisted as to form of the whole a letter T. The wire was thus retained in the duct without a ligature. The secretion was caught by a funnel covering the wound, and terminating in a graduated tube. The fluid obtained continued to present the digestive properties of the pancreas. While fasting, no secretion took place. The latter began during the first hour after food had been taken, and reached its maximum in the second or third hour, then fell, to increase again between the fifth and seventh hour, then again sank, and finally disappeared in about fifteen hours. There was complete suppression during vomiting. Division of the vagus produced no effect, while irritation of the centric end was followed by suppression. Division of the pancreatic nerves proper, which accompany the arteries, produced a continued and very abundant secretion of normal juice, and the gland became red and œdematous. Woorari poisoning produced an increased secretion.

5. *The Lymphatic System.*—By Prof. F. von Recklinghausen. (Stricker's Handbuch, der Lehre von den Geweben, 2d part, 1869.) As a result of the pressure under which the blood streams through the vascular system to the various organs, the tissues are continually permeated with serous fluid which serves in part for their nutrition, in part for the preparation of their respective secretions. The fluid requires to be rapidly changed in order not to become altered in its composition by contact with the various tissue-elements it encircles. But the passage of blood-serum into the tissues would cease so soon as the pressure in the latter was nearly equivalent to that of the blood, were there not a constant drainage effected by a system of canals, not directly subjected to the pressure of the blood-vessels. This canal system is effected by means of the lymphatic system which is only so far in connection with the blood-vessels as indirectly to derive its

fluid from them, and as to finally empty again at the end of the system into the blood-vessels. The beginnings of the lymphatics are in connection with the capillaries, in which the blood-pressure is high; whereas the terminal portions open into the main trunks of the venous system, i. e., in that portion in which the blood-pressure is at its minimum. To this difference in pressure is due the circulation of the lymph.

There are two classes of lymphatics, viz.: 1. The system of derivative canals or lymphatics proper; and, 2. The intesterstitial spaces containing the fluid which directly surrounds single elements of organs after its transudation from the blood-vessels. In birds and the mammalia the lymphatics form tubes whose walls correspond in structure to those of the veins, but differ somewhat in form, in that the numerous valves give to them a varicose appearance. In amphibia, on the contrary, they form interspaces between separate organs, which, when artificially distended, swell up to form large irregular sacs. These are really nothing but interstitial holes without walls proper, and are bounded by fasciæ and thickened connective tissue covering organs, while the inner surface of the cavities thus formed is lined by a single layer of epithelium. These lymph-sacs communicate together, by means of microscopic openings, so as to form a continuous system of cavities.

Fixed types, such as we find in the arterial and capillary blood-vessels of individual organs, can only be imperfectly demonstrated in the lymphatic system. We find the greatest irregularities, both as regards the distribution and form of these vessels. The same differences again make their appearance in the lymph-capillaries. Even in the mammalia we find them in certain organs in the form of interspaces, though generally in both amphibia and mammalia they form tubes. The arrangement of the capillary lymphatics as regards the blood-vessels is of special interest. The larger lymphatics run along sometimes close to the artery and vein, sometimes isolated from them, while the smaller lymphatics and capillary tubes, in the rule, run at as great a distance as possible from the blood-capillaries. This arrangement affords the most suitable form of drainage. All fluids which transude from the capillaries must first pass through the tissues to reach the lymph-capillaries. Moreover, in all membranes which have a free surface covered by epithelium we always find the lymph capillaries lying beneath the blood-capillaries; while the latter are situated just beneath the epithelium, the lymph-capillaries do not even reach into the upper layer of the connective tissue.

As to the structure of the lymph-capillaries in specimens injected with a nitrate-of-silver solution, an inner epithelial layer can be demonstrated. So far they possess a special membrane, but not a homogeneous structureless membrane, as was formerly supposed. In order that absorption of chyle may take place, it is essential that the walls of the chyle capillaries be not everywhere perfectly closed, but possess orifices communicating with the tissue of the mucous membrane. Such orifices have in fact in certain lymphatics already been demonstrated, through which, even during life, small bodies may pass into the calibre of the vessels. If we remove upon a cork ring a portion of the centrum tendineum of the diaphragm in a perfectly intact condition, and bring upon the abdominal surface a drop of milk, the reception of the milk-corpuscles into the lymphatics may be observed with the aid of the microscope. The milk-globules run together at certain points, and form eddies as they dip down into the lymphatics lying beneath the surface. The openings through which this movement takes place are nearly round, allow only two or three milk-globules to pass, and, as can be demonstrated by staining with silver, are formed by gaps (lücke) between the epithelial cells. They usually lead perpendicularly into the lymph-capillary, though sometimes the canals pass obliquely into the lymphatics. An injection of milk, blood, or of fluids containing insoluble substances in suspension, when made into the abdominal cavity, are absorbed and produce beautiful net-works into the centrum tendineum. Dybkowski succeeded in filling lymphatic net-works by the absorption of colored fluids from the pleural cavities in the dog, and found in injected preparations openings between the epithelial cells. In many epithelial layers, even where the lymphatic do not approach the surface, sharply-defined gaps have been observed between the cells by means of silvered preparations. These, in perfectly fresh specimens, we often fail to find, and then again they appear in great numbers; but after death, or after mechanical stretching, they are always numerous. The irregularity in the appearance of these passages in perfectly fresh specimens, perhaps, may be explained by assuming that they are transitory formations. The proof is wanting to demonstrate that they are really openings, as no one has ever shown that solid particles can pass through them. There is the same doubt regarding the appearances which are often found in silvered preparations of the lymphatics. Sometimes we find gaps between the epithelial cells, and sometimes these fail totally, so that the question at present stands thus: In certain lymph-capillaries stomata my be positively demonstrated, in others openings must occasionally exist (especially during absorption). but it remains yet to be decided whether these stomata are identical with the gaps that have been described.

As regards the relation of the lymphatics to the surrounding tissues, Van Recklinghausen maintains that the connective tissue, whether going to make up organs or found interstitially between specific tissue-elements, is traversed by fine canals which communicate directly with the lymphatics. In many organs these canals form net-works, so that fragments having a stellate appearance resemble the connective-tissue corpuscles, but these corpuscles are not directly attached to the walls of the lymphatics, as has been supposed, but are situated in the calibre of the plasmatic canals (saft-canälchen), so that they can pass directly into the lymphatics. The plasmatic canals do not possess a special wall, are not tubes therefore, but are rather hollowed out of the amorphous matter which glues together the fibres of connective tissue. They are neither simple fissures between the fibres of the connective tissue, nor are they, on the other hand, enclosed tubes, formed of membranes.

When we stain fresh connective-tissue organs by impregnation with a silver solution, it is only the solid portion that takes the color; gaps and canals remain free, so that the lymphatics and blood-vessels become prominent, and in the connective tissue occur uncolored stellate fissures, which are evidently gaps (lücke), not empty, however, but containing the connective-tissue cells, and a variable quantity of fluid. They are evidently plasmatic canals, and may be shown to be in direct connection with the lymphatics—1. By silvered preparations, where the direct passage of the canals into the lymphatics may be seen; and 2. By injections of insoluble substances into the lymphatics, which frequently pass with ease into the tissues, giving rise to a diffuse coloration. Natural injections with chyle present the same pictures, from the net-work arrangement about the central chyle-capillary, to the uniform pervasion with chyle of the entire villus.

This open connection between the canals and the lymphatics enables the latter to take up substances from the former; that the lymph-stream really takes this course is shown by the behavior of the villi during chylification. Though it has not yet been directly observed, it is in the highest degree probable that the cell-elements of connective tissue may pass from the canals into the lymphatics. Now, if we may regard the plasmatic canals as the origin of the lymphatics, they would naturally form the channels for the tissue-fluids proper, while the lymphatics furnish the receptacles, which again take up and carry off the superfluous fluids out of the tissues. Both systems may be scanty in tissues, which like tendons are more permanent, and are pervaded with a moderate amount of nutritive fluid; or the lymphatics may be very abundant, and wide in proportion to the combined calibre of the canals in tissues, in which the stream of the nutritive fluid is very rapid, as in the mucous membrane of the intestine; or finally, the proportion may be in favor of the canals, and in this case the tissues are very soft and succulent, the fluid changes slowly, and the condition is perhaps thereby particularly suited to cell-formation. In this category belong the interstitial connective tissue, the connective tissue enveloping organs, the serous and synovial membranes.

Since the passage of red and colorless corpuscles through capillary walls has been observed under certain circumstances, it is possible that the plasmatic canals stand in the same relation to the blood-vessels as the lymphatics. That such communications normally exist is rendered probable by the presence in lymph not only of white but red blood-corpuscles. Herbert found that, by increasing the entire volume of the blood, by injecting either blood or milk into the jugular vein, blood-corpuscles, or in the corresponding case, milk-globules appear in the thoracic duct. Quite recently Dr. Böhm has seen in the synovial membranes silver pictures, showing the same connection of plasmatic canals with blood-capillaries as have been demonstrated in regard to the lymphatics.

6. *Formation of Bone.*—M. Goujon (Recherches Expérimentales sur les propriétés physiologiques de la Mœlle des Os. Journal de l'Anatomie, 1869, No. 4) has succeeded in giving rise to the formation of bone, by transplanting portions of the marrow of long bones beneath the tissues. The younger the marrow, and the more vascular the tissue into which it is transplanted, the better the chance of its giving occasion to osseous productions. Goujon's experiments indicate that the consolidation of fractured bones may take place after removal of the periosteum. In the fractured bones (humerus) of fowls which are normally destitute of marrow, the latter made its appearance in the external and internal callus during consolidation. No union took place between the fractured extremities in rabbits after removal of a portion of the marrow.

7. *The Regeneration of Epithelial Formations.*—By Julius Arnold. (Virchow's Arch., vol. xlvii., part ii., p. 168.) J. Arnold has sought to establish experimentally the source of epithelium in the formation of new skin from granulating wounds. His observations were made upon the cornea, the tongue, and the epidermis (web) of the living frog, and upon the mucous membrane of the hard palate, and upon the scalp of the dog. Arnold failed to confirm the prevailing notions regarding the formation of these cells, by scission of preëxisting epithelial cells, or by genesis from connective-tissue structures. In precisely those points in which the regeneration of epithelium was most active, epithelial cells with multiple nuclei, or in the act of division, were most rare. When the wound was left to itself, the formation of epithelium took place at the periphery, thence advancing toward the centre. Yet, when this was prevented by the removal, in a granulating wound of the hard palate of the dog, of all the peripheral tissue, including the periosteum, the formation of central islets of epithelium was not prevented. The same results were obtained in wounds upon the scalp of the dog. Arnold likewise failed in finding the

so-called formative cells derived from the connective-tissue corpuscles which are held to precede the formation of the epithelial cells. Nor did he find any support for the opinion that the epithelial cells are derived from the leucocytes, which abound upon granulating wounds. Numerous as these were, they always maintained their peculiar type, and never displayed any of those metamorphoses which were likely to impart to them the character of epithelial cells.

Arnold describes the following as the results of his observations as to the plan of new epithelial formation. The surface first becomes covered with a finely-granular substance, containing in great numbers molecules perceptible only with high powers. At those points where the new formation begins, this substance becomes changed into a glassy mass, and is to be regarded as protoplasm. Next, the phenomena of segmentation take place, delicate lines traversing the mass, dividing it at first into larger, then into smaller segments, which are in part round and in part angular. In these subdivisions shining granules (nucleoli) make their appearance, around which subsequently delicate contours form. These, becoming gradually better defined, invest the nucleolus with a distinct nucleus. At the same time, in the clear substance of the divisions quite fine granules make their appearance, most marked, at first, on the outer circumference of the nucleus, and then advancing outward. The nucleus likewise becomes finely granular, but at a later period clears up, and contains only a few large granules. Sometimes, however, Arnold thought the nucleoli made their appearance before segmentation of the protoplasm. Sometimes, in a single large division were contained several nuclei and nucleoli, recalling the so-called endogmous variety of cell-formation. These, however, seem really due to an imperfect segmentation of the protoplasm.

8. *New Origin of the Sense of Taste.*—(Lussana's Pathological Observations on the Nerves of Taste, *Arch. de Physiologie*, vol. xvii., Nos. 1 and 2.) Lussana confirmed the generally-entertained opinion regarding the destruction of the sense of taste upon the posterior portion of the tongue after section of the glosso-pharyngeal, and upon the anterior portion, after destroying the lingual branches of the fifth pair. Pathological observations have shown, however, that sensation may persist with complete abolition of the sense of taste anteriorly, and that in like manner the gustatory sense may remain after paralysis of the trifacial. Lussana reconciles these conflicting statements, by maintaining that it is the chorda tympani which presides over taste in the anterior portion of the tongue. Pathology and direct experiment both prove that the gustatory sense is directly dependent upon the integrity of this nerve, and that its destruction involves the loss of the sense of taste in the portion of the tongue supplied by the lingual branch of the fifth pair of nerves.

9. *Hair turning White in a Single Night.*—By M. Brown-Séquard. (Arch. de Physiologie, 1869, No. 3, p. 442.) M. Brown-Séquard relates the following experience: His beard, he says, is black, or rather a dark brown, and has been for some years sprinkled posteriorly with gray. In the beginning of August, 1862, he noticed, while contemplating himself in the glass, some white hairs in the anterior part of his beard. He counted them. There were five on one side and seven on the other. Then he pulled them out. Two days afterward three more appeared on one side, and two on the other. This experiment he repeated many times, and found that black hairs became white in from two to five days, others, meantime, beginning to whiten in the neighborhood. As he was in ordinary health, and suffering from no mental emotion, and as there was no

appreciable cause save his age to produce the phenomenon, he argued that there can be no doubt of the possibility of hair changing very rapidly from black to white, probably in less than a single night.

10. *Influence of the Vagus on the Vascular System.*—By Dr. William Rutherford. (*Journal of Anatomy and Physiology*, May, 1869.) To test the assertion of Schiff, Moleschott, and Lister, that powerful irritation of the cardiac end of the divided vagus arrests the heart's action, while gentle stimulation quickens it, Rutherford opened, in his experiments upon animals, the larynx anteriorly, so that he could determine the weakest current, which, when applied to the recurrent laryngeal, was capable of producing movements in the arytenoid cartilages. Having settled this point, he diminished still further the strength of the current, and then proceeded to stimulate at intervals the lower end of the vagus. The strength of the current was then increased very gradually, so as to give the influence of weak currents a fair trial. Rutherford found that, if there was no struggling, no quickening of the heart followed. A much more powerful influence was required to slow the heart than to throw the laryngeal muscles into action. None of the so-called "too-powerful" currents in ordinary cases were capable of arresting the heart's action for more than a few seconds. The influences which prompt the heart to contract become so powerful that stimulation of the heart, be it ever so strong, fails to prevent it. He concludes that the vagus is an inhibitory, and in no sense a motor nerve of the heart.

Rutherford confirmed the experience of Ludwig and Cyon, that, when the vagus is divided, and the cranial end stimulated, the blood-pressure falls, owing to the dilatation of the abdominal blood-vessels, effected through the medulla oblongata and the spinal cord. It is supposed that the superior cardiac branch of the vagus is thrown into action when the heart is overloaded with blood, in order that, by the dilatation of the blood-vessels ensuing, the resistance to the heart's contractions may be diminished. When the vagus is divided, the heart's action is accelerated, a result usually attributed to the loss of its inhibitory power. Rutherford attributed the rapidity of the heart's action rather to increased blood-pressure, which has its origin in the contraction of the gastric blood-vessels (when these are distended during digestion), following division of the vagus in the neck.

11. *Terminations of Nerves.*—Lipmann (Virchow's Arch., Bd. 48, H. 2), by employing fresh preparations of the cornea of the frog, and staining them with chloride of gold, believes that he has succeeded in tracing extremely fine terminal branches of the nerves, directly into the nucleoli of the corpuscles of the cornea, and into the nucleoli of the cells of the posterior epithelium.

In the salivary glands, Pflüger (*Stricker's Handbuch*, etc., part ii., 1869) has followed the axis cylinder of nerves directly into the epithelial cells of those organs. Frankenhaüser, in *Die Nerven der Gebärmutter*, *Jena*, 1867, traced the finest terminal branches of the nerves of the uterus into the nuclei of the uterine muscular fibres. J. Arnold has found that a similar arrangement exists in the organic muscles generally (Von Stricker's *Handbuch*, *Gewebe der organischen Muskeln*, part ii., 1869).

Miscellaneous and Scientific Notes.

THE sad news comes to us from abroad, of the death on the 7th ult., at his home in Edinburgh, of Sir James Y. Simpson. There was probably no one in the profession more widely known and esteemed than Sir James Simpson, and the simple announcement of his death will cause, at least in this country, a very general and unaffected sorrow throughout the entire ranks of the medical profession—so many of whom have not only listened to his teachings, and shared his generous hospitalities, but have been benefited by the advances and discoveries in science of which he was the author.

He was born in Bathgate, Scotland, in 1811, and was educated at the University of Edinburgh, where in 1832 he received the degree of M. D. In 1840, he was elected Professor of Obstetrics in the university; and it is with this branch of medicine that his name has been especially identified, and to his labors, more, perhaps, than those of any other one man, is the Edinburgh school indebted for that prominence so universally conceded to it. In 1847, he first employed ether, for the purpose of producing anæsthesia during parturition. This was several months after the anæsthetic property of ether had been fully established in this country. About the same time Sir James Simpson discovered that chloroform possessed this power in a still more remarkable degree, and it is by this improvement in medical art that his name has become so well known the world over. In 1849, Dr. Simpson was elected president of the Royal College of Physicians of Edinburgh; in 1852, President of the Medico-Chirurgical Society; and in 1853, Foreign Associate of the French Academy of Medicine. In 1856, he received the Monthyon prize of two thousand francs, awarded by the French Academy of Sciences, "in consideration of his services to humanity, by the introduction of anæsthesia into the practice of midwifery, and the discovery of the anæsthetic properties of chloroform;" and, a short time before, he received the knighthood of the Royal Order of St. Olaf, from King Oscar of Sweden. Sir James Simpson's professional writings are numerous, and are known throughout the world, having been translated into nearly every European language. In January, 1866, he was created a baronet in recognition of the service he had rendered by the discovery of chloroform, and he received the honorary degree of D. C. L. at Oxford the same year. He was president of the Department of Health in the Social Science Congress, at Belfast, in September, 1867. In October last, the freedom of the city of Edinburgh was presented to him, and the speech which he made on that occasion, in response to the address of the Lord-Provost, drew forth from Dr. Bigelow, of Boston, a somewhat unmerited criticism, the reply to which appeared in this JOURNAL for March.

DEATH OF PROF. LORDAT.—This once celebrated professor of the Medical Faculty of Montpellier, called by Trousseau "the illustrious Lordat," died on the 25th of April, in the ninety-eighth year of his age. He was the first to describe Aphasia (Rev. Pér. de la Société de Médecine de Paris, Decembre, 1820, p. 317), which he termed *alalia*, and ascribed to a defect of coördination of the muscles used in the act of speaking, without, however, attempting to specify the brain-district whose damage might cause loss of articulate language. Eight years subsequently he himself became aphasic. Before his attack he lectured without notes, and was remarkably fluent after his recovery, he was not only incapable of lecturing extemporaneously, but even of delivering from memory lectures already written, and he was always obliged to read them. Prof. Lordat held spiritualistic doctrines.

THE chair of Surgery in the University of Maryland, made vacant by the resignation of Prof. Nathan R. Smith, has been filled by the appointment of Dr. Christopher Johnston, of Baltimore.

W. F. PECK, M. D., reports to the Iowa State Medical Society (*Medical and Surgical Reporter*, April 9, 1870) an interesting case of extra-uterine gestation, in which the fœtus, apparently of about four months' development, was delivered through the rectum. The mother made a good recovery.

Through the kindness of several of our subscribers, we have received accounts of the meetings of a number of medical societies, and notably full reports from the States of Alabama and Minnesota, for which we desire to express our acknowledgments, and regrets that the great demands upon our pages prevent us from publishing even an abstract of these interesting reports.

In the *Chicago Medical Examiner* of May, Prof. E. Andrews, M. D., has a paper on the Relative Danger from Anæsthesia by Chloroform and by Ether, in which he gives statistics of 209,893 cases. These statistics are taken from hospital and private records, and so far may be deemed reliable. They of course cover but a very small percentage of all the cases of induced anæsthesia, nor do they comprise more than a small share of the recorded cases of death. These statistics, as they stand, give 43 deaths out of 117,078 chloroformizations, or a ratio of 1 to 2,723; while from ether, the deaths recorded are 4 in 92,815 administrations, or a ratio of 1 to 23,204. Of course no one will accept these conclusions as absolutely demonstrative, but it is evident, we think, from a study of the tables, that the danger from chloroform is relatively much greater than from ether. Dr. Andrews records his belief that, in England and this country, probably not one-fifth of the deaths occurring from anæsthesia are published. The data which we have at hand are not sufficient to warrant any exact approximation of this proportion, but we are sure that very many deaths from chloroform are never made public. Our readers are already familiar with our views on the impropriety of the use of this anæsthetic.

Clinics for Mental Diseases in Great Britain.—Following the example of the late Prof. Griesinger, of Berlin, several clinics for the study of mental diseases have within the last three years been established in Great Britain. The first (we believe) was inaugurated in the winter of 1868, in connection with the medical school of the University of Cambridge, by Dr. McKenzie Bacon, the superintendent of the County Asylum, at Fulbourn, close to the ancient seat of learning. In the spring of 1869, the students of Guy's Hospital had the advantage of attending a psychological clinic given by Dr. Thompson Dickson, the *ad interim* lecturer on mental diseases in the medical school of the hospital, and resident superintendent of St. Luke's Hospital. During the summer of 1869 a course of clinical instruction on mental disease was given in Glasgow, by Dr. Alexander Robertson, who was in the United States in 1868, and afterward published his "Notes of a Visit to American Asylums."

GRADUATES IN MEDICINE FOR 1870.—(Continued from page 323, May No.)

Indiana Medical College,	20
St. Louis Medical College, St. Louis, Mo.,	43
Missouri Medical College, St. Louis, Mo.,	29
College of Physicians and Surgeons, St. Louis, Mo.,	2
New Orleans Medical College,	27
Willamette University, Oregon,	7
Medical College of Ohio, Cincinnati,	36
Cincinnati College of Medicine and Surgery,	19
University of Louisiana, New Orleans,	74

By an error in the report of graduates in our last number, we gave the Rush Medical College of Chicago credit for only sixty-nine graduates. The actual number graduated was one hundred and thirty.

DR. LOCKHART ROBERTSON.—The April number of the *Journal of Mental Science* comes to us without the name of Dr. Lockhart Robertson as one of its editors, a position he had occupied for eight years, having in 1862 been unanimously appointed by the British Medico-Psychological Association to conduct their journal, a duty he has discharged with zeal and distinguished success. The loss to the Association and to journalism is, however, Dr. Robertson's gain, and is occasioned by his appointment as Chancery Visitor of Lunatics to succeed the late Sir W. Charles Hood, M. D., whose death took place early in the year. Dr. Robertson's honors are well deserved. No one has worked more ardently and successfully for the advancement of medical psychology, and he has gained much reputation as an asylum administrator.

DR. HENRY MAUDSLEY remains sole editor of the *Journal of Mental Science;* and in his hands it will hardly lose any of the high character it gained when under the management of Dr. Bucknill or Dr. Robertson. Dr. Maudsley has lately been appointed to the chair of Medical Jurisprudence in the University Medical College, London.

ILLNESS OF SIR THOMAS WATSON.—This venerable and eminent physician, so well known in this country by his work on the "Practice of Physic," has been seriously ill. During the cold weather he was attacked with severe muscular and neuralgic pains in the back and one thigh, which confined him to bed. When it was supposed he was convalescent, he got inflammatory congestion of the lower part of both lungs. By the last accounts, the symptoms had amended, and his friends hope for recovery.

By advices received since the above paragraph was in type, we learn that Sir Thomas Watson has quite recovered.

CHAIR OF HISTORY OF MEDICINE IN THE MEDICAL FACULTY OF PARIS.—Dr. Charles Daremberg has been elected to this new chair in the Paris School of Medicine. His competitors were Drs. Lorain, Bouchet, Maurice, and Raynaud, all eminent men. The professorship is a private foundation—by a legacy of a Paris physician, who died last year.

Dr. Daremberg's great work "Histoire des Sciences, Médicales, comprenant l'Anatomie, la Physiologie, la Médecine, la Chirurgie, et les Doctrines de Pathologie Générale," founded on seventy-five lectures on the general history of the medical sciences, and delivered at the College of France in the years 1864–'67, is now complete, the last volume having just been published by Messrs. J. B. Baillière et Fils. The first volume treats of the history of medicine from the earliest period to the time of Harvey; and in the second it is brought down to the present day. In erudition it is a wonderful work, its author having devoted himself to the study of his subject since 1839, and travelling for more than ten years throughout Europe for the purpose of reading, copying, and collating the various Latin, Greek, and other manuscripts. The great epochs or movements of medical doctrines are all clearly and instructively set forth, and make a very interesting work. The New Sydenham Society would do well to publish a translation.

APPOINTMENT TO THE CHAIR OF EXTERNAL PATHOLOGY AT THE FACULTY OF MEDICINE OF PARIS.—Dr. Chauffard has been appointed Professor of Internal Pathology (practice of medicine) in the Medical School of Paris, in Dr. Lasègue's place, who was transferred recently to that of Clinical Medicine. His opponent was Dr. Potain. It is noteworthy that Dr. Chauffard is an active opponent of the materialistic views held by most of the profession. He is said to be a devout and practical Roman Catholic.

THE LITERATURE OF NASTINESS.—We have received several works, original and translated, lately published, concerning matters touching on very delicate ground. We believe that the investigation of no subject, directly interesting the moral and physical well-being of either sex, should be avoided simply because it is distasteful and repelling. We have no sympathy with such prejudice or prudery. But when it becomes necessary to expose evils of at best doubtful fitness for the public eye, and of a nature at all times abhorrent to the pure-minded, the motive should be beyond suspicion, and the treatment within the strictest limits of scientific inquiry. We regret that the least ill we can say of the works in question is, that they are sensational, and have no practical bearing on the filthy questions they refer to. They should be left severely alone. It is, we presume, in reference to this class of books that the American Medical Association adopted the following preamble and resolution at their recent meeting:

> *Whereas*, certain so-called medical works have been published which are injurious to the reputation of the profession:
>
> *Resolved*, That any person signing his name as author of such work shall be refused membership in this Association.

HOSPITAL CONSTRUCTION. By SIR JAMES Y. SIMPSON. [Medical Press and Circular, October 6, 1869.]—The following paragraphs from a letter of Sir James Simpson to the *Scotsman*, relating to the construction of a proposed new infirmary in Edinburgh, are equally applicable to hospital construction elsewhere:

1. That in the past history of medicine numerous chemical emanations and vapors have from time to time been brought

forward and temporarily lauded, under the idea that, by their disinfectant, antiseptic, or air-purifying powers, they would render the air of wards of hospitals, etc., more healthy; but none of them have stood for any length of time the test of experience. In the end of the last century, an Edinburgh graduate got a parliamentary grant of thousands of pounds for the discovery of one which is now never used.

2. That, several years ago, Dr. Lemaire, of Paris, as well as Declat, Kuchenmeister, Bottini, and others, first in their writings advocated the use of carbolic acid in medicine and surgery, on the ground of its being a destroyer of low germ-life, and that Dr. Lemaire more especially proposed its use in hospitals as calculated, among other things, to prevent pyæmia —one of the most common causes of death among surgical patients. (See Lemaire's "Book on Carbolic Acid," 1863, and second edition, 1865, p. 364; Declat's work on ibid., p. 24, etc.)

3. That, in the great hospital of the Hôtel Dieu, of Paris, M. Maissoneuve, one of the most distinguished of living French surgeons, used, from 1861 onward for a succession of years, carbolic acid in dressing the wounds, etc., of his patients, though he has now abandoned it for, I believe, alcoholic and other applications. But I am not aware that it was observed to diminish in any degree the prevalence of pyæmia among his patients.

4. That, since Prof. Lister adopted and wrote on the use of carbolic acid in 1867, it has been employed extensively in most of the surgical wards of the Royal Infirmary of Edinburgh. During the eight years from 1859 to 1866 inclusive, before the introduction of carbolic acid here, the death-rates (chiefly from pyæmia) among the limb-amputations amounted to forty per cent. During the two years 1867 and 1868, this dreadful mortality rose to fifty-three per cent. (See the official returns in the *Edinburgh Journal of Medicine* for June, 1869, p. 1106.) This increased death-rate was not, I believe, directly owing to the introduction and free use of carbolic acid in the wards during these last years, for such death-rates are liable to intermittent changes from time to time; but, at all events, the increased mortality took place despite the employment of the specific safeguard which Mr. Syme now suggests as fitted to modify the opinions of our architects in the construction of the new hospital.

5. That, if Mr. Syme really believes his proposition to be true—namely, that the new infirmary might be safely built as a block or blocks of several stories in height, *because* the air of its wards could always be kept pure by a chemical nostrum or

effluvium—then assuredly it was utterly unnecessary for him to have pleaded, as he has so ably done, for the removal of the institution from its old and restricted to its new and more spacious site; inasmuch as there was, as every citizen knows, plenty of room in the former locality for one or two great and tall-mansioned hospitals of several hundred beds each; and much money and much disputation might consequently have been spared to the community.

6. That, in all our palatial British hospitals with four hundred beds or more, the mortality in limb amputations is, on an average, as high as 40 in every 100, while in all the British hospitals of 100 beds or less, the data of which were a few years ago collected officially for a report to the medical officer of the Privy Council, the mortality is as low as 17 in the 100, making the enormous difference of 230 lives in every 1,000 human beings operated upon—a startling fact, which, with many others, shows that there is in the construction of hospitals always imminent peril in the aggregation of the patients, and safety only in their segregation.

7. That, in the construction of our new infirmary, the great disinfectants and antiseptics that we should alone depend upon are abundance of space, abundance of light, and, above all, abundance of fresh, pure, and ever-changing air to every patient in every ward in the hospital—a result that will probably be attained most cheaply and certainly by leaving the present Watson's Hospital, with its numerous class-rooms, bedrooms, etc., for the administrative part of the institution, and erecting upon the ground above and below this central building a series of village or villa hospitals or wards, furnished with all the latest and best sanitary improvements.

Hippophagy in France.—The consumption of horse-meat in France is taking a wide extension. A society has been formed for propagating the use of this food, and new stalls have been opened in all the larger cities of France, such as Rheims, Troyes, Toulouse, Marseilles, Sedan, Bordeaux, etc. Efforts are being made to introduce the use of horse-flesh into the provinces of Algeria, which so often suffer from famine; but the natives manifest so strong an attachment to their steeds that it is hardly to be expected that they will adopt the idea in a general manner. The consumption of horse-meat in Paris, which in 1868 amounted to 505 horses, or 226,000 pounds of flesh, for the months of September, October, and November, has attained, during the same period of this year, an amount of 683 horses, or 273,200 pounds; thus showing an increase of 178 horses, or 47,200 pounds, in Paris only, and during the comparatively short period of three months.

Perforation of the Rectum by a Bougie.—At a recent meeting of the Pathological Society of this city, Dr. Sands presented a specimen of a melancholy interest, from the fact that the intestine was perforated in an attempt to dilate a stricture of the rectum. The patient, a gentleman past the age of forty-five, consulted him two years ago with a stricture of the rectum, from which he had suffered for three years. A year before he came under notice he had been under the care of another surgeon, who, after dividing the sphincter ani, dismissed him cured. Failing to find the relief which he expected, he consulted Dr. Thomas, who recognized the difficulty, and sent the patient to Dr. Sands. The latter gentleman found a very tight and obstinate stricture about three inches from the anal orifice. It was only after two weeks had elapsed that an ordinary olive-pointed urethral bougie could be passed. Larger ones were used after a time, and these gave place to rectal bougies, and finally one was constructed of gutta-percha, ten inches in length, with the curve corresponding to that of the intestine, having an olive-point, stiffened by a watch-spring. This was introduced, on an average, twice or three times a week. During his absence from town, Dr. Sands intrusted the case to the care of a medical friend, who, however, was not in the habit of introducing it as he had done—its whole length. On one occasion the patient became aware that it was not inserted as far as he had been accustomed to have it, and informed the attendant of the fact. He became so importunate that the instrument was passed its full length. No difficulty was encountered, and no undue force was used. Immediately after it was withdrawn, and before the patient left the house, he complained of pain in the lower part of his bowels. As this was not an unusual occurrence, no special attention was given to it, and he was simply advised to take a little laudanum. He jumped into a stage and rode down town, but soon began to feel very badly, and was seized with a rigor. He then returned home and immediately sent for the physician. That gentleman saw him the same day, Thursday, and on Friday Dr. Markoe was called in consultation, and it was not until the Saturday evening following—forty-eight hours after the accident—that Dr. Sands visited him. It was then evident that peritonitis existed; the diagnosis of perforation of the rectum was made, and death occurred on the following morning at four o'clock.

On examining the rectum after death, the stricture was found to be five inches in length, and caused by a condensation of the tissues of the gut at that point. As had been previously diagnosticated, no cancerous disease was found. About ten inches from the anus there was found a perforation through healthy tissue, and corresponding in size to that of the point of the instrument. There was an ancient adhesion of the peritoneal surfaces of the sigmoid flexure, in such a way as to render the angle at that point very acute. This inflammation was thought to have been caused at one time when the passage of the instrument by Dr. Sands had been attended with very sharp abdominal pains. After this he had used the instrument with more caution. It was very evident, taking this view of the case, that while the whole length of the instrument could be passed previously without danger, after the adhesion occurred it was very easy to produce the perforation.—*Medical Record.*

CHARLESTON, S. C., *March* 22, 1870.

Editor of New York Medical Journal.

DEAR SIR: The number of your Journal for December, 1869, has just come under my notice, and I find in it a review of my report on the yellow fever of 1862, in Wilmington, North Carolina, by Dr. William George Thomas, a resident physician of that city. Dr. Thomas had courteously informed me, by letter, in October last, of his intention to controvert some of my statements, and I had replied to him. My letter so fully meets, in advance, all he has said, that I will beg the favor of you to give the correspondence a place in your Journal, as my reply to the review.

I will simply illustrate the justice of the remarks I have made to Dr. Thomas, in reference to the danger of setting the recollections of men, concerning events seven years old, against the records of the hour in which they transpired, by alluding to the statements in Dr. Carr's letter. From memory Dr. Carr has furnished information in which neither the names nor the dates correspond with my statements. I took my notes from the hospital-books, in the presence of Dr. Custis, and perhaps also of Dr. Carr, for there were several of the hospital staff present and aided my search.

Again, Dr. Thomas sets the records of Oakdale Cemetery in opposition to my statements. Whether these records have any reliability, I do not know; the panic was so great and general, that many of the public offices were left to take care of themselves, and in my visits to that cemetery I never saw any living men present except the grave-diggers. But, be that as it may, here, as in the case of Dr. Carr's recollections, the names given by the records are not the same as those I give, and therefore may or may not belong to the same individuals—I don't know.

Dr. Thomas's description of the topography, sanitary condition, drainage, prevailing diseases, etc., so completely corresponds with my own, that I have cause for self-congratulation at the success of my efforts at attaining to strict truth in my narrative.

With these remarks, and the correspondence, I submit the case to the jury of public opinion.

With sentiments of high respect, I remain your obedient servant,
WILLIAM T. WRAGG.

(Correspondence.)

DR. THOMAS TO DR. WRAGG.

WILMINGTON, N. C., *October* 10, 1869.

MY DEAR DOCTOR: The NEW YORK MEDICAL JOURNAL for August, 1869, was placed in my hands a few weeks since.

With the greatest interest I read your article on the Epidemic Yellow Fever, which prevailed here in 1862. Having accumulated some facts on the subject, with which some of your statements come in conflict, I at once set to work, when a leisure minute would allow, to gather still more; and the result is, that every case reported to you by Drs. Anderson and Schonwald, as having occurred prior to the Kate's arrival, occurred from several days to as many weeks subsequently to that date. I cannot agree with you in regard to the natural drainage and the water-level of this city, neither do I agree in all the statements relating to the sanitary condition of the city, so far as comparison with the years both preceding and succeeding 1862, up to 1868, is concerned. The cleanliness of Wilmington in 1862, up to the breaking out of the fever, was not worse certainly than in the other years referred to.

I have been throwing my ideas together, for the purpose of setting these facts in medical history right, and I am sure you will not disapprove of it when you have seen what I may have to say. I suppose Schonwald was moved by a desire to make facts corroborate the doctrine of domestic origin, as I am not disposed to suspect him of motives more culpable. Dr. Anderson reports partly from hearsay, and in one or two instances exhibits a striking deficiency in memory as to facts, and their connection with exact dates. As to the question of the spontaneity of the disease here in 1862, and the views in favor of importation held by others, I prefer not to express decided views hastily. I shall only controvert some of your views upon the local and general sanitary condition of the city, and then show that the cases, on which you rely to prove the existence of the fever prior to the Kate's arrival, are untenable. All the cases of Schonwald, Kling, Lutterloh, Mrs. Hansly, Lieutenant Davidson, Georgia Weeks, and Harry Smith, either did not have yellow fever at all, or those that did, had it long after that date.

The review proposed is only in an imperfect state at this time, and I may not be able to complete it in some weeks. I can only devote moments between daily duties to its preparation, and of course it will, for this reason, require time to complete it.

I feel that it is due to one who is so justly entitled to and receives the highest consideration and the sincerest esteem of which I am capable, to say this much, before I proceed to the publication of my article. I would be glad to lay it before you, before doing so, if it were possible; but Dr.

Dunster is now having a map of the city made, with particular localities designated, and urges me to forward the paper at an early day.

With sincere regard I am, dear doctor, very truly your obedient servant,
WILLIAM GEORGE THOMAS.

DR. WRAGG TO DR. THOMAS.

CHARLESTON, S. C., *October* 14, 1869.

MY DEAR DOCTOR: I am afraid you did not read my article, on the "Epidemic Yellow Fever in Wilmington in 1862," carefully, or else that I have so awkwardly expressed myself as to have given impressions I did not seek to convey. I did not write with the intention of advocating a theory, but only to state facts. This I tried to express when I penned the paragraph you will find at the bottom of page 491 of the NEW YORK MEDICAL JOURNAL. That I have a decided opinion, adverse to the importation theory, I admit. But I was not directed to give my private opinion, in that report, when the surgeon-general ordered me to write it, but to state the facts. I obeyed the order literally, and carefully abstained from introducing any theoretical views. What Dr. Schonwald's views may be I do not know, for I never heard him express any. All that I know of the opinions of the resident physicians is what I state in the second paragraph of page 479. The first information I got, calculated to throw doubt on the generally-received opinion that the Kate had brought the fever, you will find by reference to the last paragraph, on page 483, was not from one of the profession, but from the inhabitants of one of the houses into which it had been introduced (it was said) by that vessel. Struck with the description given of the illness, death, and cadaver of one who, they stated, had died at a date anterior to the arrival of the Kate, or about that time. I inquired who had attended. On being told that it was Dr. Schonwald, I called on him, and asked to be allowed to see his books. He kindly offered to send me extracts of such facts as he could find recorded there, bearing on the point. His own language is transcribed, and this, he assured me, was an extract from his book. Dr. Anderson's dates were given me after he had taken time to collate them, and his books were at hand for confirmation; and as the facts themselves had but just transpired, it seems to me that no attempt, at this late day (seven eventful years after the transactions), to recall the memory of these events, can damage the hold taken on the judgment of any fair reasoner by the statements there made. Dr. Anderson's descriptions of localities I was competent to verify, and they were so much in accord with what I saw that I cannot, for a moment, doubt that the rest of his narrative is equally in accordance with truth. Nothing short of extracts from his books giving different dates from those he gave me, verbally, at the time they were fresh in his memory, can invalidate their truth at this late date. I maintain, therefore, with all becoming respect, but not the less positively, that no mere circumstantial evidence, gathered at this late day, can invalidate the fresh recollections at the time.

With regard to the topography of Wilmington, I am responsible for the description given, and do not think I have over- or mis-stated the local peculiarities or the then condition of the city. You will, perhaps, remember some of the statements you made to me yourself, in regard to the sanitary condition of the city in past years, in conversations held previous to your illness. It was from these conversations that I penned the paragraph you will find on page 482; and my recollection goes even farther, and reproduces to my mind the remark made, either by yourself or one of the other resident physicians, that the cases they had been looking upon as jaundice, some of which died in three or four days with high febrile symptoms, they afterward, when the fever was fully developed as yellow fever, were induced to consider as early cases of that disease.

You also differ from me in regard to the sanitary condition of Wilmington at the date I was writing. This statement greatly surprises me. Such a state of things as I saw cannot, surely, be assumed as the normal condition of your city. My description is not exaggerated. I speak of what I saw, and smelled, and felt. It cannot be that the garbage scattered all over the surface—the water in the cellars and on the surface of the ground, etc.—can be found there in good seasons. It must be unusual for the large pond on the east of the city to burst its bounds, and display its slimy bottom to the scorching sun. It surely never happened before that the curtain of trees between that pond and the city was cut down, and left free space for the fresh-water emanations from that extensive sheet of water.

I said above that I had my theory of yellow fever; I repeat that I have, and that, as a great fact in the causation of that fearful disease, I reckon local causes, just such as I detail in the paper in question. But these causes do not make up all the sum of the great aggregate. I do not even exclude from that aggregate such influences as the arrival of vessels with their cargoes may exert. But to all these I add another, too much overlooked at this day, perhaps on account of its invisible, intangible, mysterious nature, but not more mysterious, probably, than any of the others. I allude to what old Sydenham (with profound insight into the operations of Nature) called the "epidemic constitution of the atmosphere." This, I believe, is the *magnum mobile*. Sanitary measures and quarantine laws may exert more or less influence on this agent—the former much, the latter (I believe) very little; but personal contagion I have no faith in. The influence of personal contagion is clearly shown in the report to be entirely without effect; that of defective hygienic measures, on the contrary, to be powerful.

It is very likely I shall never see your paper, for I do not take the NEW YORK MEDICAL JOURNAL, and, if I do see it, I shall not probably answer it. If you confine yourself to refuting my opinions, I pretty certainly shall not; and very likely I shall let it pass, even if the facts are questioned. But in the latter case I may be tempted to reply (if I see the article), though I trust that may not be necessary. I feel no vocation for controversy, particularly with one for whom I have conceived so much regard, and I trust I may not be drawn into it.

Please accept, dear doctor, the assurances of my highest regard and esteem.

WILLIAM T. WRAGG.

HAY-FEVER CAUSED BY VIBRIONES.—Helmholz says, in *Virchow's Archives*, that since 1847 he has been attacked every year, at some time between May 20th and the end of June, with a catarrh of the upper air-passages. These attacks increase rapidly in severity; violent sneezing comes on, with secretion of a thin, very irritating fluid; in a few hours there is a painful inflammation of the nose, both externally and internally; then fever, violent headache, and great prostration. This train of symptoms is sure to follow if he is exposed to the sun and heat, and is equally certain to disappear in a short time if he withdraws himself from such exposure. At the approach of cold weather these catarrhs cease. He has otherwise very little tendency to catarrhs or colds.

For five years past, at the season indicated, and only then, he has regularly succeeded in finding vibriones in his nasal secretions. They are only discernible with the immersion lens of a very good Hartnak's. The single joints, commonly isolated, are characterized by containing four granules in a row; each two granules being more closely connected, pairwise, and the combined length equaling 0.004 mm. The joints are also found united in rows, or in series of branches. As they are seen only in the secretion which is expelled by a violent sneeze, and not in that which trickles gradually forth, he concludes that they are probably situated in the adjoining cavities and recesses of the nose.

On reading Binz's account of the poisonous effect of quinine upon infusoria, he determined to try it in his own case. He took a saturated neutral solution of quiniæ sulph. in water =1 : 740. This excites a moderate sensation of burning in the nasal mucous membrane. Lying upon his back, he dropped 4 centim. of the solution, by a pencil, into each nostril; moving his head meanwhile in all directions, to bring the fluid thoroughly into contact with the parts, until he felt it reach the œsophagus. Relief was immediate. He was able, for some hours, freely to expose himself to the heat of the sun. Three applications a day sufficed to keep him free from the catarrh, under circumstances the most unfavorable. The vibriones, also, were no longer to be found.

The experiment was made in 1867; and was repeated at the first recurrence of the attack in May, 1868, preventing the further development of the attack for that year.—*Scientific American.*

Animal Vaccine.—From a discussion on this subject, which took place in the French Academy of Medicine (reported in *Gazette Médicale de Paris*), we learn that at a previous meeting M. Guérin endeavored to exonerate human vaccina from the two charges made against it, namely, its degeneration and its alliances with syphilis. In the present number he discusses the respective titles of human and animal vaccina to the preference of medical men. His object is partly to refute the arguments of M. Depaul, the warm partisan of animal vaccine virus.

The Jennerian or human vaccine virus is composed of two distinct elements which the animal vaccine virus does not possess; it is the spontaneous cow-pox transmitted to man under the form of pustules, and then, when it has acquired the elements of this double origin imparted to all humanity, the human vaccina is the spontaneous cow-pox modified by the human organism, as it were humanized. Animal vaccination, on the other hand, consists in the artificial inoculation of the cow-pox to the heifer and from the heifer to man, without the intervention of the human element. Now, this mode of vaccination is open to the charge made against human vaccina, namely, that by successive inoculations to the heifer the virus becomes attenuated. M. Guérin showed that the virus of glanders was deprived of its contagious and ulcerative character after a few successive inoculations.

Let us see how the two vaccinæ differ as to their evolution. The points sustained by M. Guérin are these: in the animal vaccina the eruption is more tardy in its appearance, ordinarily coming out from the fifth to the tenth day. When once out, it runs more rapidly through its stages; the pustulation lasting only three or four days, and its virulence only three days, namely, from the fifth to the seventh day. These propositions are to-day well substantiated by facts, and nobody will deny them. As to the power of preservation of the animal vaccina, it is so feeble that the keepers of inoculated heifers invariably refuse to deliver virus in tubes or plates. M. Depaul stated the peculiarities of animal vaccina as follows: "It is the opinion of all those who have experimented with the new method (animal vaccination) that the first manifestations are more slowly exhibited; frequently we find no result on the third, fourth, fifth, sixth, and seventh day, and even still later, however, we should not despair. The tardy appearance of the eruption is one of the characteristics of this vaccination, and, while it is a rare exception with the human virus, it can be considered as the rule with the animal virus. It is also common to observe in the same individual, that all the pustules do not appear at the same period, while some become visible on the third and fourth days; others, no longer expected, show themselves from the fifth to the eight day."

The longer stage of incubation evidently denotes less virulence: thus, in cholera, the premonitary diarrhœa can scarcely be observed in the rapidly-fatal cases of the first days of the epidemic, while it becomes gradually larger as the choleraic form of virus loses its intensity. The same signification is derived from the shortness of the next period, that of eruption. The ephemeral existence of those mild eruptive fevers, varioloid and varicella, proves it conclusively. In the animal vaccina the virulence is limited to the short lapse of three days, when the lymph is perfectly transparent; while in the human vaccina it lasts six and seven days, and is turned to account even in the scabs; thus the inferiority of the animal vaccine virus in power seems to be well established.

The Academy of Medicine of Belgium, an advocate of animal vaccination, after an investigation of the matter, reported as follows: The action of the vaccine virus, modified in its passage through the human organism, compared with that of the cow-pox directly inoculated from the heifer to the child, is about equal from the first to the fourth transmission. It even seems that this action is more powerful in proportion as the transmissions succeed one another; it remains to be seen at what generation it becomes modified. If we look into the cause of this difference of activity, we are led to believe that in the first migrations, when it is getting humanized, the vaccine virus has acquired more virulence, is better adapted to the ground in which it is expected to grow, a property which is probably weakened by further transmissions.

Direct experiments are decidedly unfavorable to the animal virus. Thus from the report of M. Depaul we see that, of seventy-eight children who were inoculated with both viruses, the animal virus succeeded in only twenty-eight, having forty-five pustules, while the human virus was successful in fifty-eight, having one hundred and sixty-eight pustules. Dr. Loillier, in a meeting of the Hospital Medical Society, expressed himself thus: "I believe it is necessary that the vaccinations made with the virus of the heifer, as it is at present practised in hospitals, should be closely watched. They do not succeed. I have not been able, in my service, to observe a single successful result. The protection which results from that practice is illusory."

The claims of the animal vaccina being so illy sustained, while, on the other hand, the merits of human vaccination have become so firmly established, our preference, until further trial, should be unreservedly given to the latter.

In his last discourse before the Academy, M. Guérin summarizes the substance of his previous discussions in several propositions, of which we shall consider only those relative to the transmission of syphilis.

The vaccine pustule is susceptible of affection by morbific influences which may alter its characteristics and substitute for its normal evolution an ulcerative process, the aspect of which presents at times the appearances of syphilis. But such cases, besides the "impossibility" of being traced up to a syphilitic origin, do not act, either in their evolution or in their treatment, according to the laws of the pathogeny and treatment of syphilis.

This proposition is based on the fact that, out of one hundred and twenty-seven children reported syphilitic by M. Depaul, from the appearance of the vaccinal pustules, all recovered without treatment and without any of the secondary or tertiary symptoms.

The causes which can vitiate the vaccinal evolution so as to give it the false appearances of syphilis are of such a nature as to exert their influence equally on human and animal vaccina. These causes, foreign to the *vaccinifère*, are either external to the vaccinated subject or inherent to its constitutional state. They can be foretold and prevented. The first part of this proposition has been proved by the conjoint statements of many observers; the second is demonstrated by the experiments of M. Jules Guérin, in which he never succeeded in producing syphilis with the vaccine virus of distinctly syphilitic *vaccinifère.*

The majority of modern experimenters profess that the agent of syphilitic contagion cannot be found in the vaccinal pustule; it resides only in the blood and never in the clear fluid of a pustule; this point, heretofore proved by facts, can be supported by a physiological conception. The vaccinal pustule and the vaccine virus appear to us, the former as an accidental secreting organ, the other as a special product of this organ, and to this fact we should give the signification granted to all the normal secretions of the animal economy, that is to say, the production of a special fluid by a special organ; therefore with a normal pustule we should have normal vaccinal product, and, *vice versa*, with an abnormal pustule we should have abnormal vaccine virus; now, the normal product of the vaccinal secretion is transparent lymph, it is why transparent lymph from syphilitic *vaccinifères* will produce only vaccina and never syphilis. In order that the contrary should take place it would be necessary to go to the source of infection, the blood, or otherwise select virus from a pustule transformed into an organism which secretes the syphilitic poison. Is not this theory confirmed by what occurs with the hard and soft chancres, the former possessing the organization of the secreting agent of syphilis, the latter lacking this organization? From this theory we derive: 1. A new confirmation of the principle which considers vaccinal lymph as exempt from any syphilitic contaminations, and of the good resulting from the method which prescribes vaccination with the lymph, to the exclusion of all the other elements of the pustule. 2. A new pledge of security furnished by the normal state of the pustule, which can become the secreting agent of the syphilitic virus only under the condition that it shall lose the shape, aspect, and organization, of the secreting agent of vaccinal virus.

We close by stating that these considerations have carried such deep convictions into the minds of certain observers, as MM. Courlieu and Sebastien, that they have not hesitated to take on themselves all the risks of syphilitic infection by vaccination with lymph taken from a syphilitic *vaccinifère.*

Two Ape-Men.—Under this title, Dr. Büchner has furnished, in a recent number of the *Gartenlaube*, the results of a personal examination of two children, both of whom belong to the class of human microcephalics, or "ape-men," as they have lately been called by Karl Vogt, in contradistinction to the "man-like apes." One of these children is at present being publicly exhibited in the towns of Germany; the other Dr. Büchner has had ample opportunities of studying from the day of her birth up to the present time.

The brain is the very organ through whose proportionally large development and high cultivation (besides his other advantages) man obtains his superiority to the beasts, and becomes what he really is—the born lord of creation. And the name "ape-men," which Karl Vogt proposes to apply to those miserable creatures in whom this peculiar attribute or requisite of humanity fails or is defective, is not wholly inappropriate. For it is not merely in the form and development of the skull and its bony appendages, but also in the whole being, deportment, manner, etc., that the microcephalic human subject resembles its nearest allies, the apes.

Vogt has sought to explain this phenomenon from the recognized principle of inheritance, called *atavism*, or the remarkable tendency shown by nearly all living creatures to resemble in certain characters not the immediate parents, but ancestors more or less remote—to develop marks or peculiarities which belonged to ancestors which have lain buried in the depths of the earth in a so-called fossil state for, perhaps, many thousand years. Nature, as it were, betrays herself, or, in her recollection of the past, falls into self-forgetfulness; or, to express it more scientifically, the vital motion which for thousands of years has inhabited a definite form, and continually urges it to repeat itself in its descendants in a similar manner, is so strong, that even in those cases where that form, through the influence of time and changed circumstances, has long since become a different one, the vital motion, sometimes, by virtue of its original tenacity, makes itself felt in the remotest descendants, and occasionally produces a so-called reversion to the original form. In a less degree we are in a position to observe the phenomena of atavism or reversion among ourselves and in our own families; for it frequently happens that individual children of a family, without any demonstrable direct cause, develop corporeal or mental peculiarities, or yet more frequently morbid predispositions, which are not found in the family itself, but which, on more accurate investigation, are shown to have been present in the grandparents, or great-grandparents, or in an ancient and collateral branch of the family.

Setting out from the laws of this atavism, Vogt has advanced the theory that the human microcephalics represent a kind of reversion to the type or the formation of that common and ancestral but long since extinct ape-like progenitor from whom, to all appearance, the present ape-type and human type, as two diverging branches of the same original stock, must in the remote past have been developed.

Much may, in Dr. Büchner's opinion, be said for as well as against this theory, but in the paper referred to he declines to discuss its merits. Supposing, however, the theory to be untrue, this would not, he thinks, in the least diminish the interest in the two children whose histories he relates; since, altogether apart from their significance in reference to the doctrine of reversion, they form a conclusive argument against all non-materialistic, or rather, non-physiological, modes of studying the life of the soul. For no one, whatever his physiological knowledge, who has once seen those wretched creatures, can believe that the mind of man is something self-existing, more or less independent of the body, or quite opposed to it.

Helene Becker, of Offenbach, near Frankfort-on-the-Main, is a child of about six and a half years old, and three and a half feet high. Her head, the proper skull part of which is about the size of a strong man's fist, has a circumference (measured over the hair) of thirteen and a half inches;[1] the transverse arc, measured from ear to ear, is six and a half inches; and an arc measured from the root of the nose to the occipital protuberance, is eight and three-quarter inches. With these Dr. Büchner compares the measurements of the head of his own son, a boy only three years of age, and sound in mind. The latter are—circumference, twenty and a quarter inches; transverse arc, twelve and a half inches; longitudinal arc, fourteen inches—thus nearly double in all directions. The difference is most marked in the transverse arc; and this is owing to the lateral compression of the skull of the idiot child, which has, viewed from before or behind, a roof-like appearance. Of a forehead there is almost nothing at all; it is so flat, small, and retreating, that one scarcely perceives it beneath the hair. On the other hand, the superciliary or brow-ridges are rather prominent, and from immediately beneath them runs the long Roman, pointed nose, in a line with the surface of the forehead. This, along with the absent or very retreating chin and obliquely-set teeth, gives to the whole countenance a peculiar bird-like expression. Moreover, the brain itself is apparently still smaller than might be supposed from the above measurements, since there is reason to believe that the bony walls of the skull are of considerable thickness.

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[1] These measurements are Rhenish, and would, if reduced to English inches, be somewhat more: 97.166 Rhenish feet being equal to 100 English feet.

Such a creature is inferior to many of the brutes. For the latter, in spite of their relatively small development of brain, often develop and satisfy in a surprising degree those faculties and necessities which belong to them by virtue of their place in Nature. The microcephalic human child, on the other hand, is not in a position to care for even the smallest of its necessities, and is a quite useless member of human society. Helene Becker can neither walk, nor stand, nor speak—can neither lay hold, nor keep hold, of any thing; and her perpetual, restless, and ape-like motions, her continual swinging backward and forward of the head and body, depend only on a morbid exaltation of the so-called reflex or involuntary muscular activity, which always rises to excess when the controlling and soothing influence of the brain is removed or injured. In a similar manner the reflex activity in the muscles of a decapitated frog, or of one whose brain has been removed, is so exalted, that even a moderate shaking of the table on which the creature lies occasions convulsions.

The continual restlessness and awkwardness of Helene Becker and her sensitiveness to tactual impressions are so great that Dr. Büchner found much difficulty in measuring her head. And this restlessness manifests itself also in her striking sleeplessness. Indeed, she hardly sleeps at all—at least, never for any length of time, and the slightest noise awakes her. She must be fed like a child of twelve months. Her body is ill nourished, and of a low temperature; the arms and legs are slender, cold to the feel, and have a blue-red appearance. Both wrist-joints show the so-called rachitic swelling; the ankles, however, do not.

The mental activity of Helene Becker may be viewed as almost as low as zero. The senses are indeed active, with the exception of a diseased eye; but they awaken no ideas. The look is staring, stupid, without expression, and cannot fix on any thing. Only the sight of glittering objects and the hearing of music—to the latter of which she is very sensitive—rouse her attention. She never laughs, but cries, and, instead of the sounds of human speech, utters inarticulate, animal tones.

The second child, Sophia L——, is not in so eminent a degree microcephalic. She was, at the time Dr. Büchner wrote, three years old. The circumference of her head is sixteen and three-quarter inches, the transverse arc ten inches, and the longitudinal arc ten and a half inches. Her forehead is not nearly so small and retreating as that of the former; but the back part of the head is very flat, and the skull, falling away on both sides, is, as in the case of Helene Becker, roof-like. At birth, the smallness of the head was not so striking. What struck Dr. Büchner was, that the so-called *fontanelles*, or openings in the cranium, which in healthy new-born infants are never wanting, were closed, as was also the case with Helene Becker. The older she grew the more manifest became the disproportion between the size of the head and that of the body. For while the latter increased, the former remained almost stationary, so that the hat she wore when nine months old can still be worn by her. This seems to be true of Helene Becker also; at least, her parents affirm, though perhaps not with *absolute* correctness, that her head has not grown at all since her birth.

The pretty-well-developed chin (which, with the upright gait, was regarded by Linnæus as the peculiar characteristic of humanity), along with the more advancing forehead and the less prominent nose, produces a very important difference between the countenance of Sophie L—— and that of Helene Becker. Despite the otherwise great similarity of both physiognomies, that of the former is less brute-like or bird-like than that of the latter. But the eyes of Sophie L—— have almost the same vacant, idiotic expression. Also, when pleasantly excited, as by calling to her, by holding glittering objects before her, or by music, she distorts her countenance to a smile. In nearly all other respects she stands in almost the same grade as Helene Becker. She is unable either to stand or walk; can neither eat nor drink without help; can grasp or retain nothing with the hands. She cannot speak, and can only cry, and utter unnatural sounds. To pain she is little sensitive, but is much addicted to anger, and is endowed with a strong predilection to biting. Her legs and arms are weak, slender, badly formed, but without trace of rachitis—are always cold to the feel, and of a blue-red color. Her sleep is bad and short; must be induced chiefly by means of opium, and she is very easily awakened. Her senses are good; she hears and sees distinctly; but her sensations seem to give rise to hardly any mental activity.

Manifestly both children belong to one and the same category of microcephalics, and differ only in degree. Probably no city or neighborhood is without a case similar to that of Sophie L——; but Helene Becker is certainly a very rare specimen of the genus to which she belongs.

Notice of M. Claude Bernard.—I read the other day some bibliographical notes about M. Claude Bernard, the eminent physiologist, which are worth translating. He was born at St.

Julien, a small village near Villefranche, Department of Rhone, to which, unlike most Frenchmen (they are absolutely indifferent to their birthplace; M. Sainte-Beuve never once revisited his, although it is only a few hours from Paris; M. Théophile Gautier once revisited his native town, and remained there forty-eight hours), he is devotedly attached. He has added to the patch of vineyard his parents possessed, and now possesses quite a large vineyard. He regularly goes there to superintend the vintage, and remains there until he has sold his wine. He retains the friendship of his old playfellows and his parents' contemporaries, and delights to grasp their hard hands when they greet him with "Good-day, Bernard, how art *thou!*" To which he replies: "Thank, *thee*, Frisé, and *thou!*" You know *thou* is used here only between intimate friends. M. Bernard's parents were extremely poor. His mother used to carry eggs to Villefranche to sell from door to door. She determined to give him a good education, and when he completed his studies she expressed a desire to see him enter a dry-goods shop. His good fortune, however, led him into an apothecary's shop. He became infatuated with chemistry, made rapid progress in it, had his attention turned to physiology, studied under Magendie, who took a fancy to him and pushed him up with all his influence. Two or three years ago M. Claude Bernard fell ill at St. Julien, and was confined to his bed for six months. His wife, who after her first visit positively refused to return to "that horrible hole," did not go to see him once during his long illness. He deeply felt this exhibition of indifference, and it is said to have been the last, the decisive wrong which led him to bring suit for divorce. Two children, daughters, issued from this marriage. You know he is now a member of the Academy of Sciences and of the French Academy, a senator, a professor at the College of France, and at the Garden of Plants, all of which places yield him an income of some $10,000 gold annually.—*Paris Correspondent American Literary Gazette.*

THE INFLUENCE OF PLEURISY IN THE PRODUCTION OF PHTHISIS.—Dr. Andrew Clark read a valuable paper on this subject at the October meeting of the Medical Society of London (Lancet, October 23, 1869). The following is an abstract of the paper:

> He described it as a mere statement of the results of inquiries into the relation of the two diseases. By phthisis he meant any disease in which there was ulceration or suppurative destruction, more or less circumscribed, of portions of the lung: hence there was "pneumonic," "tuberculous," "fibroid" phthisis, etc. By pleurisy he meant cases in which a large amount of lymph was exuded, rather than serosity, and in which, after the acute stage is passed, there was just enough local in-

flammation left behind to prevent repair. If asked what relation there was between the two diseases, the general answer would be, none; and he referred to the opinions of Baillie and Laennec, who denied the connection, and of Louis, in which the most complete denial was to be found. Louis said that dry pleurisies were caused by tubercle; but he disbelieved the converse, when he must have seen cases in which there was dry pleurisy of the base of the lung, without tubercle. Baglivi contended for the opposite view, and he and others belonged to what may be termed the "inflammatory" party, which conceded all to inflammation. This party was defeated by what may be termed the "constitutionalists," whose doctrines, as typified by Louis's opinions, were generally accepted, and have been handed down to recent times. In 1848, however, Addison made his assault on them, and showed that tubercular lungs might be the ultimate result of pneumonia—a state of things which he designated as pneumonic phthisis. Addison, indeed, laid the foundation-stone of that reform in the nomenclature of lung-disease which, if not accomplished, is at least near at hand. The author had traced out this connection, and in 1858 he began to keep an eye upon all his cases of pleurisy, having them under constant observation for years after the attack, and the proportion issuing in basic lung-disease of a phthisical character is large enough to take them out of the category of coincidences. There were two modes of evolution of phthisis in these cases: By a uniform invasion and hardening of the lung by fibroid tissue. This was not necessarily followed by the breaking up of the lung; it was the cirrhosis and interstitial pneumonia of authors. The other was an irregular distribution of fibroid matter, which alters the local circulation, so as to induce tubercular deposit. The conditions most likely to be followed by phthisis were those in which a large amount of lymph was effused, and where local irritation persisted after the acute attack subsided. When the fibroid deposits begin to soften, tubercle appears. The signs were generally those of condensation, with retraction; the symptoms, ineffectual cough ending in vomiting, difficult expectoration, with grayish sputa, presently fetid; the treatment, local blistering, iodine inhalations, and iodide of potassium and alkalies internally. The moral of the history of these cases is mainly this: When a patient comes under observation with pleurisy, to vigorously treat him till all appearance of local disease has gone, and not to lose sight of him till then.

EXTRACTION OF A CATHETER THROUGH THE UMBILICUS.—M. Foltz recently read at the Lyons Medical Society (Medical Times and Gazette, January 8, 1870) a very interesting case that had been under his own and M. Petrequin's care.

A woman, the mother of several children, applied to him, stating that, six days before, when between six and eight weeks advanced in pregnancy, abortion had been effected by means of a female catheter. In passing in the instrument, however, it escaped from the hand, and could not be recovered, nor could it be found amid the discharges which ensued on the operation. Careful exploration with the finger, the speculum, and Simpson's instrument, carried in every direction within the cavity of the uterus, failed to discover any metallic object. Examinations of the bladder and rectum were attended by the same result; and, as there was neither fever nor pain present, expectation was advised, it being also suspected that the instrument might have passed out without the patient's knowledge.

It was not until four months later that she again presented herself, still having all the appearance of health, but complaining of a tumor at the umbilicus. This did not project much, but it was hard and rounded, and reducible on the slightest pressure, to reappear immediately—just as a commencing umbilical hernia. M. Foltz, however, felt certain that it was produced by the extremity of the catheter; and, in fact, by passing a finger of the one hand into the cervix uteri, and placing the other hand over the umbilicus, the body could be moved to and fro pretty much within the axis of the upper aperture of the pelvis. M. Petrequin, now consulted, advised the immediate extraction of the foreign body, although, thus far, it had given rise to no unpleasant symptoms. Its great mobility, the impossibility of determining the precise seat of its lower extremity, and the fact of its entire disappearance during horizontal decubitus, rendered this no easy matter. The total disappearance of the object, as if by enchantment, when the woman lay down, was indeed very remarkable, and after she had stood up it required that she should walk and fatigue herself for at least an hour before the umbilical tumor reappeared and the diagnosis could be pronounced as certain. During the treatment preparatory for its removal, on several occasions its presence could not be determined. As a preliminary step, the uterine cavity was efficiently dilated by the introduction of compressed sponge, twice daily during a week. But, in spite of such dilatation, the lower end of the catheter could not be felt, nor the exact portion of the organ in which it was located determined, the upper end retaining its mobility so as only to be vaguely felt. A renewed dilatation for six days longer was effected, when it had to be suspended for eight days in consequence of the menses appearing. When these had ceased it was renewed for another three days, and it was then ascertained that the lower end of the catheter was lodged in front of the anterior wall of the uterus, or rather in the utero-vesical partition. Between the uterus and bladder, on a level with the superior orifice of the cervix, the anterior lip of which was notably thickened, the index-finger, introduced into the cervical cavity, or merely placed at the bottom of the anterior *cul-de-sac* of the vagina, was able to feel, through a considerable thickness of tissue, but still plainly enough, the end of the catheter. In order to remove the instrument, the woman was placed standing against an article of furniture, and, while M. Foltz fixed between two fingers the end that projected at the umbilicus, M. Petrequin incised the whole of the anterior lip of the cervix, sometimes employing long straight scissors, and sometimes a narrow long bistoury with a rounded end, and having its blade guarded with diachylon. To the surprise of both, the incision did not allow of the extremity being reached, although it extended through the whole lower lip and higher up than the cervix. Endeavors were in vain made during two hours to reach, its mobility and the depth of its situation rendering wounding of the bladder a possibility. Next day, after an ineffective renewal of the attempt, the removal of the body by the genitals was given up, and an incision comprising the skin and aponeurosis was made over the upper projecting end at the right of the umbilical ring. At the same time, the instrument was thrust from below upward, so as to project through this incision. A thread was passed through its eyes in order to retain it, and a renewed attempt was made to remove it through the genital organs by pushing it toward the uterus. This, however, could not be accomplished, and it became evident that the entire instrument had passed through the uterine wall, which had closed and cicatrized over it. It was then resolved to remove it by drawing it upward with a torsion movement, in order to disengage it, but, on raising it up, the womb and bladder were dragged with it, and such severe pain and nausea caused as almost to produce syncope. The greatest precaution was therefore required, in order to gradually break up the adhesions. The instrument was then withdrawn until its end was nearly reached, when the ring soldered on to it here rendered some enlargement of the incision necessary. No bad symptom followed the extraction beyond some rather severe abdominal pain at first; and the catamenia have since appeared.

Sir JAMES YOUNG SIMPSON, Bart., M. D., D. C. L.

(From a late Photograph.)

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Art. I.—*Intra-Uterine Medication, its Uses, Limitations, and Methods.* By E. R. Peaslee, M. D., LL. D., Professor of Diseases of Women in Dartmouth College, etc.

Applications to the canal of the cervix uteri are universally accepted by gynecologists at the present day as indispensable in uterine therapeutics. But applications to the proper cavity of the uterus, though known to Hippocrates,[1] have, after many centuries of almost entire disuse, been but recently revived, and must still be considered *sub judice*, so far as their uses and the methods of application are concerned. To the latter I invite the attention of the Academy. And, since the term intra-uterine includes both kinds of applications, I shall style the latter endometrial,[2] and the former endotrachelial.

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[1] See an historical sketch of uterine injections, translated from Conheim, by Dr. J. Kammerer, in the American Journal of Obstetrics, for February, 1869.

[2] I propose the term *endometrium* (ἔνδον, within, and μήτηρ, uterus) as the name of the membrane lining the cavity of the uterus; and *endotrachelium* (ἔνδον, and τράχηλος, neck), of that lining the canal of the cervix. And hence I derive the adjectives endometrial and endotrachelial. The latter Greek word has already found its place in our science, in the name

Endometrial medication, therefore, and as applied in affections of the uterus not immediately consequent on parturition, is the subject of the following paper.

ENDOMETRIAL MEDICATION.

Injections, and the introduction of unguents and strong solutions into the proper uterine cavity, have been highly recommended from authoritative sources, during the last ten, and still more during the last five years, in the treatment of uterine catarrh and endometritis; and have been far more frequently resorted to in this country than abroad.

Like other new suggestions, they have done both good and harm; and some eminent practitioners maintain, far more of the latter than of the former. At any rate they are, it is believed, at present often used in conditions not at all justifying them; and in such cases at least only harm can result.

It is the writer's opinion that, heretofore, endometrial applications have so generally been made in a random and uncertain manner, and without a clear idea of the precise conditions which indicate them—though he is familiar with eminent exceptions to this statement—that they have thus far produced, on the whole, more evil than good. This he believes, however, to be no longer a necessity; but that, in rightly selected cases and applied by the best method, endometrial applications are of the highest value in the treatment of certain endometrial affections.

And, with but slight allusion to other writers, the following view of the subject is submitted, partly as founded on his observation, but more especially on his own experience in the use of these applications, up to the present time.

It is necessary at the outset to distinguish two methods of endometrial medication—viz., by *injection*, and by *ingestion*. The practical importance of this distinction will become more apparent as I proceed. I first consider—

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of the trachelo-mastoid muscle. Endometritis and endotrachelitis are legitimate derivatives from the terms proposed above. Intra-cervical (of Latin composition) is synonymous with endotrachelial; but endo-cervical, as well as endo-cervicitis, is a barbarous combination of both Latin and Greek in the same word.

A.—Endometrial Injection.

It is unnecessary to state, except to contrast this with the next method of application, that by endometrial injection is meant the introduction of a fluid into the uterine cavity by means of a syringe. The manner of application, and not the fluids themselves (not the *injecta* or injections), distinguishes this from the other method. It is well known that injections of pure water into the uterine cavity are sometimes accidentally made by patients intending to inject the vagina alone; and instantly followed by the most intense pain, and fainting, and sudden collapse.

This accident is not, however, always or very generally productive of such symptoms; since it can hardly be doubted that it occurs far more frequently than they are met with. But an exceptional effect, of so serious a phase, should certainly deter us from injecting the uterine cavity, even with water, without a valid reason for so doing.

Besides, it should be known that pure water is not the least irritating of fluids, as seems to be generally supposed, in its action on either mucous or serous membranes. A weak solution of common salt (℈j to ʒj of salt to Oj of water) is far less irritating than pure water, when applied to the eye, the nasal passages and pharynx, to the rectum, the bladder, the vagina, the uterine cavity, or even to the peritonæum. I have frequently verified all these statements; and the last, perhaps, as frequently as any of the rest, in using injections of salt-water into the peritoneal cavity, in cases of septicæmia occurring after ovariotomy.[1]

The occurrence of such accidents therefore, from the injection of pure water into the uterine cavity, does not prove that other injections would be equally, or perhaps at all, objectionable. Experience alone can decide the question.

But how are the effects sometimes produced by endometrial injections of simple water to be accounted for?

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[1] The reason of this is found in the fact that all the secretions in the human body naturally contain common salt, in variable proportions; and a weak solution of common salt approximates therefore more nearly to the natural secretion, in each case, than pure water.

1. If the water is several degrees colder than the endometrium, the direct impression upon it may both be very painful, and a strong and painful contraction of the uterus be also excited.

2. The sudden and forcible distention of the uterine cavity may also both directly produce pain, and lead to painful contractions.

3. In certain cases of hyperæsthesia of the endometrium, the mere contact of water at the same temperature as the membrane may produce grave symptoms, from its irritant effects in such conditions.

4. It has been maintained by some writers that the water injected passes through the Fallopian tubes, in some cases, into the peritoneal cavity, and then perhaps produces peritonitis.

Admitting the possibility of such passage, should one or both of the tubes be abnormally dilated, and the water be injected with great force, I think this accident to be so very improbable that it hardly needs to be taken into account; and certainly not if the injection is made in the precise manner advised in this paper. The Fallopian tubes have a diameter of only one-fiftieth part of an inch at their junction with the uterine cavity; and, even though the cervical canal had not been previously dilated, the water could far more easily return from the uterine cavity by the side of the syringe than be forced through these passages.[1]

5. Finally, it has been suggested that the severe symptoms from the endometrial injection of water may be due to the fact that both air and water are forced into the uterine vessels.

The admission of air and of the water into a vessel might occur, provided the point of the syringe directly enters a vessel of considerable size, and an excessive amount of force is applied.[1] But, unless varicose veins exist on the endometrium, this result is scarcely possible; and, if it should occur, the very small amount of water injected would be harmless, though an exceedingly small amount of air might not be so. No operator would be likely to apply the force necessary to produce such results.

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[1] The experiments of Vidal de Cassis, Hennig, and Klemm, also prove this. But very great pressure may cause the injection to enter the uterine vessels.

If, then, we intentionally inject water into the uterine cavity, we may best guard against the effects I have mentioned by adopting the following precautions:

1. Let the water be at blood-heat or very slightly less.

2. Introduce it very slowly, carefully, and without force, so as not to distend the uterine cavity much or suddenly. And here it must be inquired how much water is required to fill the normal uterine cavity; only ten to fifteen drops in the virgin, and from twenty-five to forty in the case of a woman who has become a mother. The slightest pressure is, therefore, evidently enough to fill the cavity if a proper syringe is used.

3. But we should also be certain to secure a return of the overplus of water injected into the cavity, through the cervical canal, by the side of the instrument; thus both preventing distention of the uterus, and also removing all chance of the passage of water through the Fallopian tubes, or of either water or air, or both, into the vessels. This, of course, implies *a previous dilatation of the cervical canal,* so as to give it a considerably greater diameter than the tube of the syringe used.

4. But even with all these precautions we may, in the hyperæsthetic condition alluded to, produce intense pain and sudden sinking, by the mere contact of warm water with the endometrium. We may, indeed, expect this result, and abstain from the use of injections in all cases, if the introduction of the sound, or the probe, into the uterine cavity, produces that intense pain, on contact near the fundus, which some regard as characteristic of endometritis.[1] If there is no special sensibility of the endometrium, injections of pure water into the uterine cavity, with the precautions just enumerated, may be regarded as safe; and I have not yet found a case in which the injection of a weak solution of the chloride of sodium (from grs. ij to grs. iv of salt to the ℥j of water) did not prove to be absolutely so. But the salt-water is sometimes objection-

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[1] This hyperæsthesia is very sure to be developed, if there is any coexisting perimetritis or parametritis, as well as in cases of acute endometritis; and in all such cases the idea of endometrial applications is not to be entertained.

able; especially if the nitrate of silver is to be applied immediately afterward.

Recapitulation.—Warm water, and especially a warm, weak solution of common salt, may be injected with almost entire safety, except in some cases of great endometrial hyperæsthesia, if we—

1. Dilate the cervical canal (in the manner explained further on).
2. Use a properly-constructed syringe (to be described).
3. Inject without force, slowly, and only one to two drachms, before stopping to let the fluid return from the cavity before more is introduced.

But what shall be said of the safety of other fluids so often injected into the uterine cavity? A strong solution of nitrate of silver, chromic acid, and the persulphate or perchloride of iron? In the first place, these substances, unlike water, may produce serious effects if taken into the circulation; and if freely injected into the uterine cavity there is a risk of this, and especially if the endometrium, as is not rarely the case, is denuded of epithelium. I must here also mention the direct irritant effects of these substances generally, upon the endometrium, and the caustic effect of chromic acid and nitrate of silver.

The danger from the entrance of powerful endometrial injections into the circulation must not be ignored; though, up to the present time, we know nothing as to the amount of these fluids respectively which may safely enter the blood. But five or ten drops of Magendie's solution of morphia, injected into the uterine cavity in certain conditions of the endometrium, produce as decided an effect as if injected hypodermically.

But, whether we maintain that the effects are produced as just suggested, or are due to chemical action, or to mere irritation, all very well know that much suffering at the time, and subsequent acute metritis, have not very seldom been produced by endometrial injections of the substances named; and that severe perimetritis has followed in their train, and which has in some instances proved fatal. Of course the risk is less the smaller the injection; and Dr. Kammerer, of this city, who probably has had as much experience in intra-uterine injections as any living physician, prudently uses but from ten to twenty drops of the concentrated solutions[1] at a time.

But it seems to be assumed still by some practitioners that the endometrium will patiently endure any amount of irritating or cauterizing they may choose to inflict; indeed, that it requires a tremendous appeal to elicit any therapeutic response.[2] This error must be unlearned before the actual danger and the true value of endometrial injections, when properly applied, can be accurately estimated. Meantime weak solutions should be used if we resort to injections at all.

But it is far better to *ingest* the remedy to be applied. We must not be misled by the fact that frequently, and in certain indolent states of the uterus, powerful injections may be made without producing any dangerous reaction; but remember that the very strong injections are, though often salutary, to be accepted up to the present time as very hazardous.

B.—Endometrial Ingestion.

By endometrial ingestion, as another method of applying remedies to the endometrium, I mean the simple operation of carrying a medicinal agent, either a fluid, an unguent, or a solid, directly in contact with and over that membrane, as paint is laid over a surface to be painted.

Ingestion is by no means a new method, and, as usually practised, presents no advantages over injection; but, on the contrary, is far less certain in its effects, though it, on the other hand, often proves quite as dangerous as injections. By the use of proper instruments, however, it may be made more efficacious than injections; and almost if not perfectly safe, when applied in the proper conditions.

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[1] Dr. Kammerer recommends the following concentrated solutions: chromic acid two parts, water one part; iodine one part, iodide of potassium two parts, water four parts; carbolic acid and water equal parts; pyroligneous acid undiluted. These are applied once in from three to eight days. His weak solutions have about one-tenth of the strength of the above.—American Journal of Obstetrics, August, 1869, p. 203.

[2] Haartman maintains that endometrial injections never produce pain by their irritating qualities, since powerful remedies may be used thus without producing severe symptoms.

It has long been known that, while simple warm water injected into the uterine cavity may produce the severe symptoms before mentioned, a very strong ointment, as of nitrate of silver, may generally be applied without any decided reaction. But the explanation of this fact has been long in receiving a practical application. If a considerable amount of the ointment is applied, it may produce as decided symptoms as the strong injections before mentioned.

The different effect, then, is due to the different modes of application. In the case just supposed, the ointment, being applied in considerable quantity, produces distention of the uterus, and thus the effects before explained; but ointments are usually applied on cotton attached to a probe—at any rate in very small quantity—and thus no reaction ensues. They are, as it were, when applied in the best manner, merely painted over the endometrial surface. The lard also prevents the medicament it includes from acting suddenly upon the membrane; but the method of application is the principal point here insisted upon, and which will be fully explained under another head. (p. 481.)

No one believes that ten or fifteen drops of blood-warm water, applied to the endometrium by a camel's-hair pencil, would, in any condition of the uterus, produce any very severe symptoms, though the water injected very often proves dangerous. And here, again, the advantage of ingestion over injection is seen. In the former there is no overplus of the fluid, no distention; but just enough used to cover the membrane and no more. I advocate this method in all cases in which it is appropriate, instead of injection, and especially whenever the strong preparations are used.

The *ingesta*, or remedies to be ingested into the uterine cavity, are:

1. Solids.
2. Fluids.
3. Ointments.

As the latter present no advantages over the fluid ingesta, I shall not again allude to them. If the best process of ingestion be adopted, they may as well be discontinued, and the fluid ingesta always take their place.

Of *solids* the nitrate of silver is almost exclusively used at the present day, as an application to the endometrium. Courty suggests that a mass of it be left in the uterine cavity, in certain conditions; and even strongly advocates this "little operation," as he calls it. It has, however, proved a very unsafe application in some instances; while in others it has become embedded in a mass of tenacious mucus, and thus been expelled some days afterward. That it ever produces any favorable effects not attained by milder agencies, the writer has no reason for believing; and will not soon again be induced thus to use it.[1]

The fluid ingesta are the same as the injecta, and are of the greatest importance. They will be particularly specified anon.

To bring this subject to a practical form, I devote the remainder of this paper to the three following inquiries:

1. When may endometrial applications—both ingesta and injecta—be made?
2. What substances may thus be used?
3. How shall they be used—i. e., how shall injection and ingestion be performed?

It is in connection with the last question that I hope to present some improvements upon the methods hitherto in vogue.

I.—WHEN MAY ENDOMETRIAL APPLICATIONS BE MADE?

The uterine affections not consequent on recent parturition, in the treatment of which these applications are especially appropriate, are metrorrhœa[2] (or uterine catarrh) and metrorrhagia. Chronic endometritis would also, by some gynecologists, be added here.

But it should be premised that, if metrorrhœa or metrorrhagia be produced by a displacement of the uterus, or by a new formation in, or projecting into, its cavity, the cause should of course be removed, if removable, before endometrial applications are to be thought of; which being done, it very generally results that no such applications are at all required. If, however, the cause cannot be removed, they may still, in many cases, prove to be of value merely as palliatives.

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[1] The *ultimate* effects of the application of strong caustics to the uterus, upon the reproductive function, are still but little understood, but are to be thought of before resorting to this class of ingesta.

[2] See an able article on the Treatment of Uterine Catarrh, by Dr. J. Kammerer, in the American Journal of Obstetrics, for August, 1869.

1. In *metrorrhœa* injections are recommended, first, to cleanse the uterine cavity; and, second, of the curative fluid applied to the endometrium. For, since the catarrh is merely a symptom depending usually on chronic endometrial congestion—though sometimes, perhaps, on chronic endometritis[1]—the treatment should of course be directed to the cure of the diseased membrane.

The first step, cleansing the membrane entirely of all the secretion, is of indispensable importance; since, if not thoroughly effected, the curative agent does not come in contact with the membrane at all. Injections of water are recommended for this purpose, but ingestion is as effectual, and, as has been suggested, is far more safe. Ingestion of the medicament that is to be applied is far preferable to injection, for the same reason.

2. *Metrorrhagia* is another condition often treated by endometrial applications; and here also the blood is first to be removed, and then the styptic applied directly to the membrane. Both these objects may be accomplished by injection alone, or ingestion alone; or the water may be injected to remove the blood, and then the styptic be ingested.

The metrorrhagia which so frequently occurs, independently of intra-uterine new formations, at the approach of the menopause, is often permanently cured by a single styptic injection. And, if properly made, I consider injection in these cases a perfectly safe procedure.

I do not myself add another condition of the uterine cavity not consequent on parturition,[2] as demanding endometrial applications. If, however, chronic endometritis can be distinctly diagnosticated from chronic endometrial congestion, it here finds a place, and is treated, like the latter, by injection or by ingestion. But, if there is mere congestion (or inflammation) and no metrorrhœa, of course no cleansing application is needed, but the curative agent is applied at once.

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[1] See Dr. J. C. Nott's article on the Treatment of Endometritis by Intra-Uterine Injections, in the American Journal of Obstetrics, for November, 1869.

[2] Of conditions following parturition, I may mention *post-partum* hæmorrhage treated by injection of the perchloride of iron, as recently, but not originally, suggested by Dr. R. Barnes; and puerperal metritis treated by injections of disinfectant fluids. Here also injections are quite harmless.

Endometrial applications may therefore be made with good results in the treatment of metrorrhagia, metrorrhœa, and in chronic endometrial congestion also, even if unattended by metrorrhœa. But the preliminary injection of water is less safe, and no more efficient than its ingestion; and the same is true respecting the stimulants or styptics subsequently to be used.

Thus endometrial *ingestion* is required, and should be resorted to, in but a *very small* proportion of the uterine cases the gynecologist has to treat; while injection should far more seldom be resorted to, and never, I think, unless preliminary to ingestion of the curative agent, in cases in which there is no other way to remove the blood or the secretion, and in cases of metrorrhagia when styptics are freely required. I would use injections unhesitatingly in these last conditions, if other remedies did not succeed, or more time could not be lost; and believe that they will here be more and more highly appreciated the better the profession understand the best methods of applying them.

II.—WHAT AGENTS MAY BE USED AS ENDOMETRIAL APPLICATIONS?

1. For cleansing the endometrium I have already mentioned warm water, and salt and water as still more safe. If there be but very slight sensibility of the endometrium, a very little soap added to the water is an advantage—especially if the secretion to be removed is very viscid. In some recent cases the detergent applications only are required to effect a cure.

2. But, before speaking of the *curative* agents to be applied to the endometrium, let me bespeak for that membrane some share of the consideration and respect which is paid to all the other mucous membranes. Nobody thinks of injecting chromic acid, or tincture of iodine, or a saturated solution of nitrate of silver, into the bladder, the vagina, the rectum, the eye, the mouth, the pharynx, or the nasal passages; and, if the last is

applied to the eye or the pharynx in exceptional cases, it is done, not by injection, but by the most careful ingestion to the precise surface requiring the application, and directly under ocular supervision.

I invoke the same regard for the endometrium, and the entire rejection of all these powerful applications till a previous use of mild solutions demonstrates their necessity, and the tolerance of the uterus; and claim that, when used, they shall be applied with the utmost care and caution.

Of the above-mentioned remedial agents, the tincture of iodine is perhaps the most valuable, in cases of metrorrhœa, being used at first of the strength of ʒ j to ℥ j water, then ʒ ij to ℥ j water, and so on up to the full strength, if required. In cases of very long standing it may be used pretty safely at first with an equal quantity of water. The applications may be repeated in from four to seven days, or less frequently, according to their strength and effects. The weak solutions may be applied every two or three days, till the tolerance of the uterus is ascertained.

The sulphate of zinc may also be used as just explained, beginning with grs. v. to ℥ j water, and going on to a saturated solution even. The same may be added in regard to alum, tannic acid, and sulphate of copper. Nitrate of silver is a valuable remedy, and is usually applied unnecessarily strong. It may range from grs. v. to ʒ j to ℥ j of water.

The tannate of glycerine, from ℈ j to ʒ j tannic acid, to ℥ j glycerine; iodine (Churchill's) ʒ ss to ℥ j glycerine; and chloride of zinc, ʒ ss to ℥ j glycerine should be added. Chromic acid has already been mentioned. It should be used, if at all, with much care, and as weak as one part acid to ten parts water at first. Finally, very rebellious cases may require the strong solutions recommended by Dr. Kammerer. (p. 471, note.)

If the endometrium is too sensitive at first to admit of the preceding applications, or becomes more sensitive from a too strong application, Magendie's solution may be applied to it with an equal quantity of water. A saturated solution of chlorate of potassa is also found to be a very soothing remedy.

In cases of metrorrhagia the persulphate[1] or the perchloride of iron (the latter being seldom used in this country) bears the palm. Maunsell's solution may at first be injected with an equal amount of water; and in urgent cases I have never met with any unpleasant effects from using it in full strngth. Saturated solutions of tannic acid and of alum are very efficacious in urgent metrorrhagia. But, without further augmentation of this list, I inquire—

III.—HOW SHOULD ENDOMETRIAL APPLICATIONS BE MADE?

Preparatory to endometrial injection or ingestion, as has already been stated,[2] we must *dilate the cervical canal.*

1. By the use of sponge-tents or the laminaria digitata.

Since I have used medicated sponge-tents (carbolized), I have discontinued the use of the laminaria.

The sponge-tent is the proper method of dilatation, if the cervix is very firm, and especially if indurated; but, if somewhat lax and easy of dilatation, the method next to be mentioned is preferable.

2. Six or seven years since, I began to use a set of steel dilators of my own devising, with the object of producing a far more rapid dilatation of the cervical canal than sponge-tents or laminaria can effect, and which I have found very satisfactory.

There are five dilators in the set, ranging from one-eighth to five-sixteenths of an inch in diameter. Each is guarded by a bulb one and three-quarter inches from its point, so that it can pass only that distance into the uterus, and therefore project only from one-quarter to one-half an inch into its proper cavity; their object being solely to dilate the cervical canal, without any needless intrusion into the cavity. The same handle is fitted to all the five dilators, and the instruments when arranged for use are ten and a half inches long (Fig. 1).

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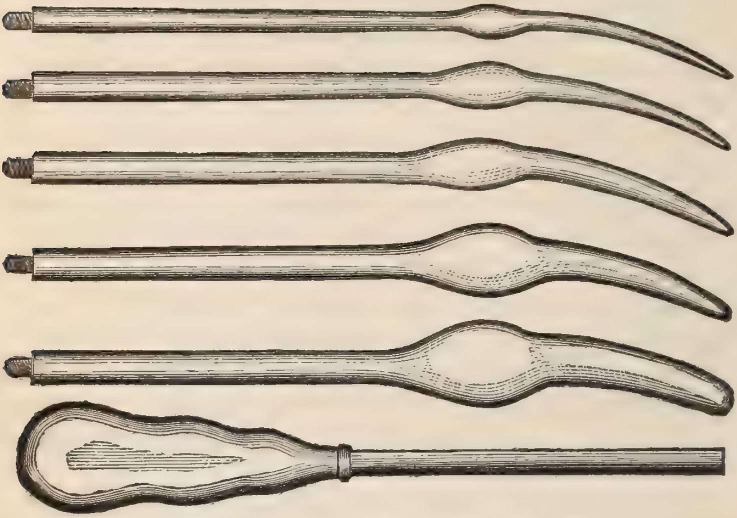
[1] These agents are the most to be relied upon in the management of alarming hæmorrhage after delivery, whether premature or not, and when uterine contraction cannot be induced or maintained.

[2] Dr. Nott, of this city, and others, have devised instruments on the double catheter plan for endometrial injections, in order to avoid the necessity of a previous dilatation of the cervix. (*See* Dr. Nott's instruments in American Journal of Obstetrics, for November, 1869.)

Without underrating the value of these instruments, I still prefer the dilatation, for reasons that will appear.

FIG. 1.



(Dr. Peaslee's Steel Dilators.—Three-quarters size.)

The patient, lying on a sofa or bed, is placed on the back with the feet drawn up. The dilator is passed along the left index-finger, without recourse to the speculum. If the cervix is quite yielding, the full amount of dilatation may often be effected at one visit in from ten to twenty minutes. If more resistant, two visits may be required on two consecutive days. But, if the cervix is quite resistant, the Nos. 1, 2, and 3, may be used, and then a sponge-tent be introduced to complete the dilatation within the next twenty-four hours. If much pain is produced, remove the instrument, and repeat the attempt two days later. In a few cases, where there was much resistance and pain at first, I have abandoned the dilator, and accomplished the object gradually by the use of carbolized sponge-tents. I have not seen any inflammation in the uterus or its appendages, excited by the dilators.[1]

[1] Dr. Kammerer, of this city, has devised a set of instruments similar to those above described. We were pursuing our investigations in this matter at the same time, without either being aware of the fact; and we both, as

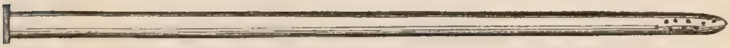
But, whatever method is resorted to, the dilatation should be carried to at least three-eighths of an inch, whether the fluid is to be applied by injection or ingestion.

A.—HOW TO PERFORM ENDOMETRIAL INJECTION.

As usually performed, this operation often fails entirely of accomplishing its results; or, if successful, also produces the undesirable effects before mentioned. Even so experienced an operator as Dr. Kammerer states that "in some instances the tube of the syringe is so firmly held by the internal orifice, that not a drop of fluid can return alongside of it" (though he always premises a free dilatation of the cervical canal), "and a certain amount of force is necessary to withdraw the instrument from the uterine canal." In such instances, the fainting and pain, before described, may of course be expected to ensue. It is therefore desirable to maintain the dilatation for any length of time required, to make certain the reflux of the fluid, and this I accomplish by the method to be described:

1. The syringe itself need hold only ℥ss to ℥j, as the uterine cavity contains not more on the average than one-twentieth of an ounce (twenty-four drops); though a somewhat greater capacity is not an objection. The tube should be six or seven inches long, and only one-fourth of an inch in diameter; and be perforated on all sides, but not at the end, by at least a dozen small holes, as shown in Fig. 2.

FIG. 2.



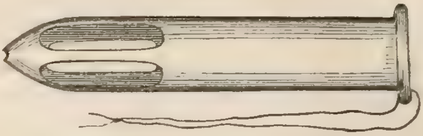
(Syringe-tube, one fourth of an inch in diameter, and six and a half inches long.)

2. I have devised a silver tube with a conical extremity, three-eighths of an inch in diameter and two inches long, with a collar at its proximal extremity (Fig. 3). One and a quarter inches from the collar commence three fenestræ, extending from that point to within a quarter of an inch of the conical end of the tube; each fenestra including one-fifth of the circumference

is natural, adhere to our original form of instruments. See American Journal of Obstetrics, for August, 1869, for drawings of Dr. Kammerer's instruments.

of the tube. There is also a very fine opening at the conical end of the tube; and a thread is passed through the collar for withdrawing it when required.

FIG. 3.



(Tube for endometrial injection.)

The patient lying upon the back, as for the dilatation, the tube just described is passed, upon the largest steel dilator,[1] into the canal of the cervix (previously dilated to three-eighths of an inch), and onward, till the collar reaches the os, when the fenestræ will open freely into the cavity of the uterus. This may be done with or without the aid of a speculum, as the operator prefers. The syringe-tube is now passed within the silver tube, and the fluid carefully injected through its fenestræ into the uterine cavity. It is better to introduce the syringe through a speculum, if we are also to use the curative agent after injecting mere water or some other detergent; but if only the latter, the whole process of dilatation, introducing the tube, and injection, may better be accomplished without it. If a very strong solution is to be injected, it is better to use a short speculum (three to three and a half inches), and as large as is easily practicable. The injected fluid is returned with certainty by the sides of the syringe-tube, there being a space between it and the silver tube of one-sixteenth of an inch in thickness; and the silver tube is retained *in situ* at will, until all unpleasant symptoms have been met by soothing injections through it, if any such arise. The normal virgin uterus is represented (to show its cavities) by Fig. 4, and the tube applied as just explained by Fig. 5.

Thus all the precautions before insisted upon (p. 469) are fulfilled, and at the same time the fluid injected is not brought into contact with the canal of the cervix at all.

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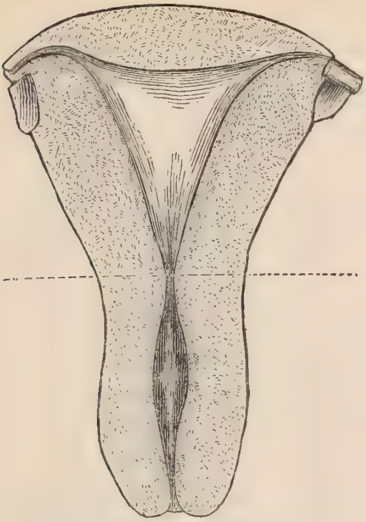
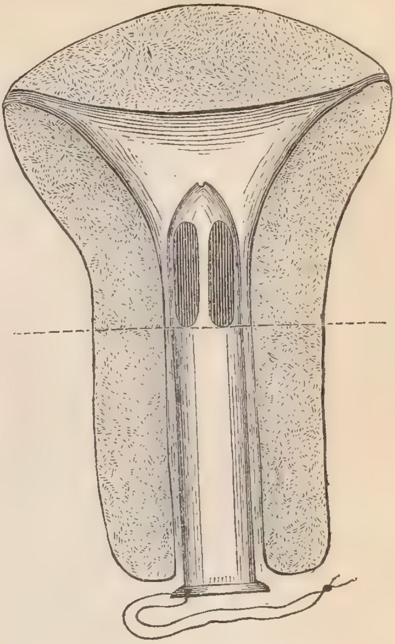
[1] Or, a wooden plug, accurately fitting the tube, may be introduced into it, and carry the tube into position in the cervical canal.

FIG. 4.



(Normal virgin uterus, showing cavities.)

FIG. 5.



(Tube applied for endometrial injection.)

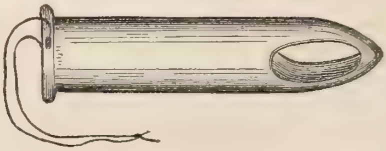
B.—HOW TO PERFORM ENDOMETRIAL INGESTION.

The usual method is described in his excellent work on Diseases of Women, by Dr. Thomas: "A probe properly wrapped with cotton-wool, and dipped into the solution to be applied, is introduced directly to the fundus, and kept there for thirty seconds to a minute" (p. 480).

Now, inasmuch as no previous dilatation of the cervical canal is here contemplated, there is no certainty that the medicament will reach the uterine cavity at all, but the probability is (and especially when the canal is still of normal dimensions) that it will not. But, if it should by mere chance do so, it will not, if there be any secretion or blood in the uterine cavity, after all, come in contact with the endometrium. Usually, therefore, little or no reaction follows this operation. But, if the canal of the cervix is somewhat dilated, and there is no metrorrhœa or metrorrhagia (when of course ingestion is not indicated), the medicament may be brought into direct contact with the endometrium; and, if one of the concentrated solutions has been applied, we may frequently have very good reason for adopting Dr. Thomas's suggestion following the above quotation: "After this, send the patient to bed, to remain there quietly three or four days."

Here as well as in the case of endometrial injection, in order to operate intelligently, we must—1. Dilate the cervical canal; 2. Cleanse the uterine cavity of any secretion; and then, 3. Maintaining the dilatation, carry the remedy fairly into contact with the endometrium. And all this may be certainly effected by means of the instruments I have already mentioned; substituting for the silver tube[1] (Fig. 3) the following modifications of it, viz.: This tube is but one and three fourths inch long, and has but two fenestræ instead of three, these being opposite to each other; commencing also one and a quarter inch from the collar, and extending to the conical end of the tube. The thread passes through the collar at the point which should be the lowest when the instrument is introduced, and thus indicates the precise position of the fenestræ (right and left), in the uterine cavity. Fig. 6 represents these modifications.

FIG. 6.



(Tube for endometrial ingestion.)

This tube being passed into the uterine cavity in the manner before explained, the medicament is carried through the fenestræ and painted over the whole extent of the endometrium at will, by a mass of cotton as large as will easily traverse the fenestræ, attached to a steel wire with the surface at its extremity roughened like a *rat-tail* file, to secure a firmer attachment of the cotton. Fig. 7 shows the instruments *in situ*, while sweeping the left half of the uterine cavity.

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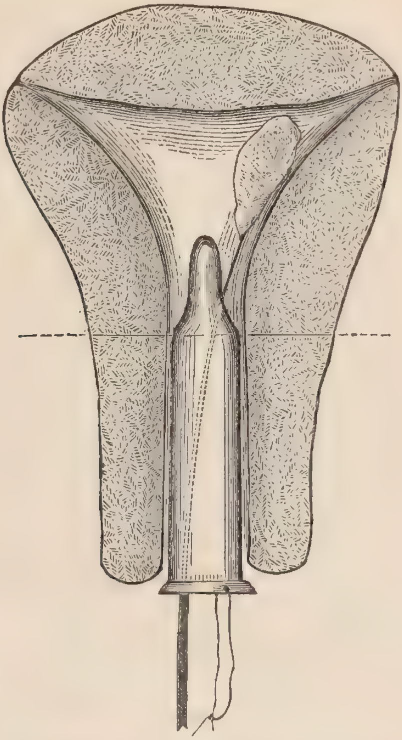
[1] This tube answers tolerably well for ingestion, but Fig. 6 much better. Both may be left open at the distal end, and be only one and a half inch long; but in that case the uterine cavity is not so well opened for this application, and the membrane is injured by the edges of the tube.

FIG. 7.



(Tube and applicator for endometrial ingestion—*in situ*.)

In cases of metrorrhœa no previous injection is required to cleanse the cavity; that object being easily accomplished by a mass of dry cotton swept over the endometrium by the applicator just described. In the same way, also, any excess of the medicament may be removed, if this is indicated by symptoms following its ingestion; or Magendie's solution may be applied as after injection. And if, for any reason, it be desirable, the tube can be kept in place indefinitely by a tampon. Obviously the method just described is equally applicable to the ingestion of solid substances into the uterine cavity, if in any case they are to be preferred.

Thus, I propose to avoid the admitted danger of endometrial injections, first by making them in a more certain and safe manner than the usual method; and second, by substituting for them another method, so far as is practicable, and which is rendered almost entirely harmless in cases requiring such applications, by the method I have proposed.

CONCLUSIONS.

1. Endometrial applications should be but very rarely resorted to by the gynecologist; being proper only in cases of metrorrhœa and metrorrhagia, and perhaps sometimes in chronic endometritis without a discharge.

2. Endrometrial ingestion, as proposed in this paper, may be accepted as perfectly safe in the cases and circumstances specified; injection is rendered far safer by the method here proposed, but is still not so safe nor so efficacious as ingestion.

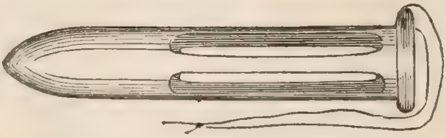
3. Injection should be restricted to urgent cases of metrorrhagia, and to certain cases of hæmorrhage after delivery; and here may be regarded as indispensable.

4. The weaker solutions should be tried in each case before the very strong are resorted to; both to prove the necessity of the latter and the tolerance of the uterus.

5. Applications to the endometrium demand a delicate surgical dexterity; and those who possess neither tact nor experience in this direction will probably produce more mischief than benefit by their use.

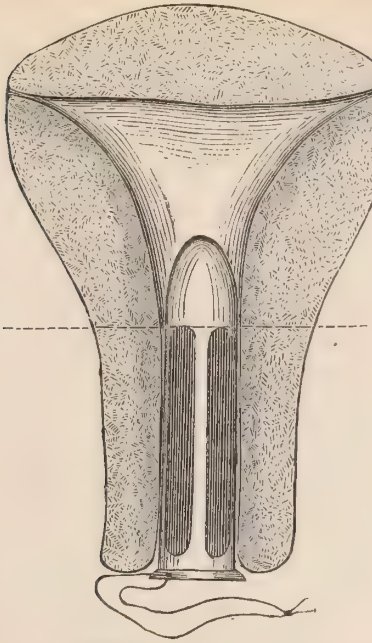
ADDENDUM.—I have also devised an instrument for endotrachelial applications, both solid and fluid, which is shown by Fig. 8; and also in position by Fig. 9.

FIG. 8.



(Tube for endotrachelial applications. For dimensions, see text.)

FIG. 9.



(Endotrachelial tube in position.)

It is one and three-fourths inch long and three-eighths of an inch in diameter, with three fenestræ, each including one-fifth of its circumference, extending from the collar one and a quarter inch toward its conical extremity. Being introduced after a proper dilatation of the cervical canal, the medicament may be applied to three-fifths of the entire endotrachelium; when, rotating the tube sixty degrees, the rest of it is uncovered and receives the application in its turn.

ART. II.—*Practical Reflections on the Nature and Treatment of Croup.*[1] By J. H. HOBART BURGE, M. D., Surgeon to Long Island College Hospital, Brooklyn, N. Y.

CROUP was doubtless known to Hippocrates, but we find no clear description of it earlier than that of Blair in Scotland, 1718. Home, of Edinburgh, also gives it a distinct place in the family of diseases in 1765. After which time it is found under a great variety of names in nearly all works on general practice; and many monographs of more or less merit have appeared from time to time.

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[1] Read before the King's County Medical Society, November 16, 1869.

Of all the titles given to this disease I prefer laryngo-tracheitis, as expressive of the fact that it is an inflammation of the larynx and trachea. In this I but conform to the view of Cullen, Home, Dickson, Peaslee, Condie, and a host of others.

This opens at once the question, What is essential to an attack of croup? and, if I were to pause for a reply, the answers which would be given, by the different members of any medical society of this size, would sufficiently indicate the necessity of a thorough discussion of the subject, and a better understanding of each other's views.

Never shall I forget the confusion of sentiment and expression which existed at a session of the New York State Medical Society seven years ago, when this subject was introduced, in connection, I think, with a paper by Dr. Bissell, of Utica, on diphtheria. I am sure a majority of those present felt a painful sense of the ludicrous. Just such a scene occurred in this society several years ago, when a creditable paper was read by Dr. Crane.

Some insist upon several varieties, and so call every attack *croup*, which has for its main feature *dyspnœa* or threateniug suffocation, which dyspnœa depends on obstruction in or of the larynx. Others restrict the title *croup* to those cases in which a false membrane exists. At first sight this would only seem to create a war of words—a confusion in our means of communicating our views to each other.

This, however, does not constitute the main difficulty; for the question is one of pathology and not of terminology.

One class of medical men hold that membranous croup is a distinct and fearfully-fatal disease, having no connection whatever with the other varieties so called; while another class contend that these varieties are only different grades and states of the same affection. Of two medical men equally well informed, one tells us that, while membranous croup is almost always fatal, the spasmodic and simply inflammatory will almost as surely recover even without treatment; the other tells us that they are in essence the same thing, and that the milder varieties, as he calls them, will, unless resolution take place, go on to diphtheritic deposits or to death without this symptom.

All agree as to the propriety of dividing croup cases into *true* and *false*, but one class restricts the title *pseudo-croup* to laryngismus stridulus[1]—a purely spasmodic affection—while another includes in this category all the inflammatory cases in which false membrane does not appear.

One or the other of these distinct views is held positively and defended earnestly by comparatively few of the profession; while the great majority either suspend judgment on the controverted points, or hold loosely and in a vacillating manner whatever views they may be said to have on the subject.

Practically, the great mass of the profession, in common with the laity, call every case croup in which an obstruction in or of the larynx (not caused by whooping-cough, œdema of the glottis, asthma, or a foreign body) gives rise to dyspnœa and threatened suffocation, and, when we are called to such a case, be the same catarrhal, spasmodic, inflammatory, diphtheritic, or false-membranous, laryngeal, tracheal, or bronchial, sthenic or asthenic, mild or severe, examine, sift, and compare the symptoms as we may, inform ourselves as thoroughly as possible as to the history of the attack, the susceptibilities of the patient, and all other points of interest—still we are anxious as to the results.

Why is this, if the distinctions are practically as clear as they are theoretically?

For the sake of perspicuity I will state my conclusions first, in several propositions, and then say a few words upon each:

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[1] In the use of this term, I do not forget that it was applied by Dr. Mason Good to that mysterious and sometimes chronic affection which the Germans call Thymus Asthma, and which Gooch denominated "child-crowing." The difference between this and the *false croup* to which I allude is, that this seems dependent upon some organic lesion affecting directly or indirectly the recurrent laryngeal nerve; while the pseudo-croup, which I would carefully distinguish from this, as well as from laryngo-tracheitis, is a transient spasmodic affection, depending without doubt upon the reflex action of some remote irritation, as dentition, indigestible food, and the like.

Proposition I.—False croup is a simple spasmodic affection, very well named laryngismus stridulus. It is rarely if ever fatal, and would hardly excite apprehension if it were always possible to distinguish it, and to feel sure that there were no inflammatory complications. It is always sudden, generally occurs in the night, is frightful to witness, and distressing to experience.

Remarks.—Those who pride themselves on great accuracy of diagnosis, would hardly allow that it were possible to confound this mild disease with true croup. Yet it is so confounded constantly, and so heroically treated, especially by the non-professional, who manage a large proportion of these cases, that a multitude of lives have doubtless been unnecessarily sacrificed, and the cases charged to the account of "membranous croup." It is true that the points of difference between this mild affection and the grave malady under discussion are sufficiently numerous and diagnostic when they can be made out, which, in the mild affection, is generally after needless medication has been resorted to. To show how easy it is to be in doubt, in real practice, in the middle of the night, when all is excitement and anxiety, and where it is next to impossible to get a light to shine into the fauces even, and where also it is quite impossible to learn whether the child was perfectly well on going to bed or not, I will quote two short paragraphs from that interesting and accurate work of Condie, on diseases of children. After having described the usual course of true croup in its different degrees of severity, he says:

> In other cases, however, the disease commences much more abruptly, and proceeds with great rapidity and violence. The patient, who retired to bed apparently in perfect health, is suddenly awoke from his sleep with a violent fit of loud, ringing cough, etc., etc.

Ten pages further on he says:

> True croup always commences gradually, the severer paroxysms never occurring until the disease has lasted for at least *some short time.*

There is no real contradiction here. I make these quotations to show how the cases apparently run together at the very time when the practitioner must decide quickly what to do. Dr. Jacobi, of New York, after making the clearest possible distinctions between the true and false, uses the following language: "This much may be stated here, and practice will admit the fact, that the affection will frequently (especially when there is no epidemic diphtheria) commence by pseudo-croup, and afterward assume a more formidable character." He says also:

> You may see in the mouth a catarrhal proliferation, or croupous condensation of the epithelium, on the tonsil a diphtheritic deposit, embedded in the tissue, on the larynx and trachea a plain croupous deposit, and in the bronchi a muco-purulent secretion, and again, under the same endemic and epidemic influence, you will find a case of catarrh, a case of croup, a case of diphtheria, a case of follicular exudative amygdalitis, in the same family, in the same week. Thus it appears that, in the long list of morbid conditions met with, catarrh on the one side, diphtheria on the other, are but the starting and terminating points, between which all the different shapes and forms may be registered according to their dignity; their modifications depending on individual local, endemic, and epidemic influences; the only form which is perhaps, and perhaps only, to be excluded, being the necrotizing diphtheria.

Again, Watson, whose work on Theory and Practice (excepting his therapeutics) is one of the best and decidedly the most popular that ever was published, so expresses himself as to call forth the following criticism from his special admirer, Dr. Condie:

> Under the head of Child-Crowing or Spurious Croup, Dr. Watson has confounded two very distinct diseases. One occurs most commonly entirely independent of inflammation in any portion of the air-passages, and consists simply in a spasm of the glottis—the results of a reflex action, through the excito-motory property of the nervous system, of various irritations of the gums, or digestive apparatus, or of the action of external agents, or mental emotions. This is the affection known as laryngismus stridulus. The other disease differs in nothing from genuine croup, excepting that the inflammation is less intense, and seldom extends into the trachea, the exudation is of a more muco-form character, seldom adhering long enough to the inflamed mucous surface of the larynx to become converted into false membrane. This form of laryngeal disease (continues Condie) has been termed *catarrhal croup—false croup*, and, by some, *spasmodic croup*. We prefer the name spasmodic laryngitis given to it by Rilliet and Barthes.

It is true, then, upon the testimony of Condie, that Watson, an authoritative teacher in the medical Israel, may and actually does confound these different affections. If this be so, can we expect greater accuracy among those who have less knowledge and less experience? At the risk of seeming prolix, I must beg you not to understand me as detracting in the slightest degree from the importance of those distinctions which are made and which actually exist between the purely spasmodic and the inflammatory affections of the larynx and trachea. I know very well that the simply spasmodic will almost always recover, that the mildly inflammatory will generally recover, and that those which go on to the formation of pseudo-membrane do generally die. I know also that when the death or the recovery has taken place, and you have the whole history of the case before you, it is almost always the easiest matter in the world to say to which class it belonged. Moreover, in exceptional cases it is easy to make an early and positive diagnosis; but the idea which I wish clearly to express in this connection is, that *generally*, when you are called to a case of simple spasmodic croup, with the imperfect history that you are able to get, and the imperfect examination that you are able to make, you cannot feel sure that there is no inflammation; and, if there be inflammation, you cannot feel sure that it will not go on to exudation, coagulation, and ultimate suffocation: and the practical point is this, that too often the *worst* is assumed to be probable, and the treatment is directed accordingly.

Proposition II.—True croup is an inflammation of the tissues lining the larynx and trachea, and sometimes extending to the bronchial ramifications. It may be superficial, involving only the mucous membrane, or it may involve the subjacent areolar tissue.

Remarks.—There is no reason to regard this inflammation as in any sense specific; it is a simple inflammation, arising generally from ordinary atmospheric causes. It does not seem remarkable that children should be more susceptible to these influences, and therefore more liable to croup, than those of mature years. Nor is it difficult to see why children at the breast are less liable than after weaning; they are better protected from cold and damp.

Stokes, Bouchut, and Guersant, speak of it as contagious, or probably so, but the verdict of the profession is against this idea. It has often been epidemic, as we might expect it to be, just as influenza is, and from like causes; but in such a fatal prevalence as that reported by Tarrand, where, within a small compass and in a short space of time, in 1827, sixty were attacked, and none escaped death, and also in those cases where good observers have pronounced it contagious, I strongly suspect the disease was a true diphtheria. It is much easier to account for so fearful a mortality upon this theory of a specific poison entering the system in a necessarily fatal dose, than to understand how so many cases of croup could occur, and have no mild case among them.

In speaking to a learned society, such as I have the honor to address, it is not necessary that I should detail the symptoms of a disease so familiar. I shall only refer to them incidentally as I proceed. The spasmodic element exists in all cases of croup, though in exceedingly variable degrees. The febrile excitement is seldom great, and sometimes hardly observable.

Proposition III.—Effusion of plastic lymph, coagulation, and consequent formation of false membrane, occur in about one-sixth of all the cases of true croup.

Remarks.—It is so common to speak of membranous croup, that it is difficult to get out of the unscientific habit of regarding the occasional symptoms here referred to as diagnostic. Bouchut, Good, Guersant, Brettoneau, and many others, so regard it. Dickson says it does not always exist even in the worst cases. Chapman often failed to find it in the cadaver. In a fatal case which I attended eighteen years ago, in conjunction with Dr. Sweet, of New York, we made an autopsy, and no membrane existed, though a more protracted and painful death from suffocation was never witnessed. Dr. Peaslee says:

> We need not, for any practical purpose, admit an inflammatory and membranous croup, any more than we should make the same distinction in regard to pleuritis or peritonitis. Croup is always inflammatory, and some cases are accompanied by the formation of a false membrane. The latter should not affect the treatment of the disease as an inflammation, but merely because of its mechanical effects. Its occurrence cannot be predicted in any case, until it is actually seen, and this is not possible in most cases, even when it is developed at the very outset of the disease.

It may occur in *any case of laryngo-tracheitis*, and our anxiety always testifies to our faith in this fact. It will not occur in five cases out of six. Dr. Peaslee is therefore right, in my opinion, in maintaining that it ought not to be included in the definition of the disease, or be expressed by its name. To regard the false membrane as diagnostic, and therefore as indicating a disease separate and distinct from inflammatory croup, is not only to erect a symptom which is sometimes present and sometimes not, into a disease, but it is practically pernicious, since it leads us to cry, "*Peace, peace*, where there is no peace," in the inflammatory cases, and to neglect, perhaps, the very means which are necessary to prevent such cases from becoming membranous.

Adults have more judgment in expectorating, and thus getting rid of the products of inflammation before they have time to become coagulated and solidified. Moreover, I suspect that an inflammation of the larynx which an adult would carry with him to his daily avocation, and perhaps never speak of to any one, would, at the impressible age of two years, cause a spasmodic complication which might destroy life.

Statistics of croup-cases are generally worthless, because you cannot tell, unless you have a minute report of each, what proportion are mild and what severe; what kind of cases were included as true and what set down as false; what influence early treatment had in preventing fatal symptoms, and in cutting the disease short before it merited to be called a severe case.

With regard to its fatality, Prof. Ware, of Boston, reports 90 per cent.; but, then, it must be remembered that he rigidly restricted the title "croup" to those cases in which that terribly fatal symptom—the formation of false membrane—had actually occurred. If all the cases of laryngo-tracheitis which he treated of had been included in the estimate, it is not probable that the percentage of deaths would exceed 40. Dr. Jacobi says 75 is highly favorable; yet it is not quite clear upon what basis he makes his estimate. Upon the best evidence which I can bring to bear on the subject—if I were to hazard an opinion—I should say that, including all well-marked inflammatory cases, not 20 per cent. are lost when treated judiciously.

Proposition IV.—The popular treatment of croup, in all its phases, has been and still is, in my opinion, severe and full of danger.

Remarks.—I by no means intend to assert that I alone treat this disease on conservative and safe principles; it is so treated by thousands throughout the world. Nevertheless, the great number of cases are vomited before the doctor sees them (often with tartar-emetic), and approved text-books on practice say "That's right, give the child another dose." Cheyne says tartar-emetic is the sheet-anchor. Dr. John Elliotson has "no doubt that he has generally failed with antimony, because he has used it timidly, and quotes almost approvingly the experience of a medical man who gave it till twenty-seven grains were swallowed, and tetanic spasms produced. In *Braithwaite's Retrospect*, January, 1857, several English physicians indorsed Dr. Elliotson's views, but Dr. Prior, of Tewksbury, says that "as the result of extensive experience he can testify" that, "if such heroic practices be carried out, the deaths from this cause would be immensely increased."

As an emetic in croup, Condie says a majority of physicians prefer tartar-emetic.

Watson begins his remarks, on the treatment of croup, with this sentence: "The three remedies most requiring consideration are bloodletting, tartarized antimony, and calomel."

Mr. President, I join in no tirade against these time-honored and valuable agents, but I believe the time has come when we have better means at command to meet the indications which arise in the course of this disease. Bloodletting, though by common consent abandoned, was almost as universally recommended even into the present century. Calomel is hardly ever given now, except in a cathartic dose; but antimony, the most dangerous of all, still clings to its false reputation. The people give comp. syrup of squills, and call it croup-syrup. If the child dies by syncope, of absolute prostration and exhaustion, as I have seen them die after domestic treatment, the friends say, "What a terrible thing this membranous croup is!" and the doctor arrives in time to give a certificate, when perhaps laryngismus stridulus is the worst name that he can conscientiously write. Dr. Horace Green says: "The injudicious use of tartarized antimony in the diseases of young children has destroyed more lives than it has been instrumental in saving." Dickson begins mildly—gives a dose of paregoric; "afterward," he says, "an emetic is demanded, and if not relieved by the first emetic and the lancet, repeat the emetic; then comes the cathartic, salines preferred." He says, "The invading stage of croup is under domestic control." Twenty years ago Prof. Ware, of Harvard University, after deploring the loss of thirty out of thirty-three cases of "membranous croup," wrote as follows: "The method in common use of treating this disease requires careful reconsideration." He also asks: "If the mode of treating croup commonly adopted does no good, are we sure it does no hurt?" Many authors advise that, besides the occasional emetic, nauseating doses of antimony or of ipecac. be continually given. This somewhat anticipates the subject of—

Proposition V.—Emetics and nauseants, as a rule, do harm.

Remarks.—The only exceptions that I would make to this rule are, that ipecac. may be given in a single emetic dose, when the stomach is full, particularly if indigestible food has just been taken, and sulphate of zinc or sulphate of copper, or some other non-depressing emetic, when death threatens from simple laryngeal obstruction, and when there seems reason to hope that the false membrane may be detached and thrown off by its mechanical action. Nauseants and emetics, except for the purposes just indicated, cannot reasonably be expected to do good; surely the spasmodic efforts and temporary cerebral and cervical congestions which attend the act of vomiting cannot lessen the laryngeal inflammation. Again, they debilitate and rapidly unfit the little sufferer to contend successfully with his great enemy, for Watson says that "in fatal cases the false membrane is sometimes found detached from the larynx, so that it might have been expelled without much forcing or difficulty, if the child could have sufficiently inflated its lungs, and the *requisite muscular power had remained.*"

Conserve, then, this muscular power. Again, emetics, nauseants, and expectorants, fill the lungs with mucus, at a time when it is extremely difficult to get rid of it, and when it can have no other effect than to act as an additional barrier to the little oxygen which reaches the blood. I have made an exception in favor of certain mechanical or non-depressing emetics in the last stages of croup, with a view to the detaching and expelling of the false membrane. I will finish here what I have to say upon that point. It is true that the dilatation of the larynx occasioned by the act of vomiting is well calculated to favor the detachment of the dreaded false membrane. Dr. Meigs, of Philadelphia, prefers for this purpose alum, which may be given in five to ten grain doses. Dr. Hubbard, of Maine, uses and has great faith in turpeth mineral, yellow sulphuret of mercury, two to three grains repeated in fifteen minutes if it fails to operate. Sulphate of copper, first introduced by Serlo, has been given by Schwabe in fifty cases, a grain and a half to four grains every hour, eight to twelve doses. Horner Koff reports ninety-nine cases, with seventy-seven recoveries, in which he used this remedy. It is also highly recommended by Berrignier, Trousseau, and Luzinsky. The sulphate of zinc is also recommended by many, dose five to fifteen grains.

Thus far, my remarks on treatment have had reference mainly to what should not be done, and I cannot sum up this branch of my subject by any words more apt than those of Prof. Ware, published twenty years ago. After the fearful experience to which I have already alluded, he proposed to treat croup without depletion (except, perhaps, a few leeches), without vomiting, without purging, without blistering, without antimonials, without ipecac., and without any of those nauseating remedies which had been usually resorted to. Although Dr. Ware was the highest living authority at that time, the world was not ready for so great an innovation.

Had the treatment which he proposed been as sound as this negative proposition, it would probably have exerted a more abiding influence upon the profession.

It has excellent features, and, as it consists of three points only, I will rehearse them: 1. Full influence of opium, combined with calomel. 2. Constant application of warmth and moisture to the neck, and of mercurial liniment, slightly stimulating. 3. Constant inhalation of the vapor of water. A vast improvement on the general practice of his day.

The course of treatment which I adopt, and heartily recommend, may be stated in a few words :

Proposition VI.—If the stomach be full, or indigestible food have been recently taken, a single emetic may be given. It is, however, in my experience, rarely required.

Remarks.—The emetic here is not so objectionable as under any other circumstances, because the form of disease which arises from reflex action after a full meal is generally spasmodic. If, however, there should chance to be inflammatory action commencing, the expectorant effect of the remedy will be mainly over before the height of the disease would be reached. Even here three to ten grains of the sulphate of zinc would probably be a better choice than ipecac.

Proposition VII.—Give a dose of bromide of potassium sufficient to quiet all spasmodic action—four to twenty grains, and repeat every six hours.

Remarks.—It is supposed this remedy may also be useful, to some extent, in retarding or preventing the deposit of false membrane, its solvent action upon such deposits being well known to the profession. Two grains in an ounce of water will liquefy a false membrane in a few hours. If restricted to its antispasmodic property only, I would never omit it, since the spasmodic element is more or less marked in all cases of true croup.

Hydrocyanic acid, first introduced by the Italian physicians, is recommended by Horace Green. Wenett gives one grain of musk every hour. Dickson relies upon paregoric. I regard the bromide as more reliable than either of these.

Proposition VIII.—Give one-half to one teaspoonful of liquor calcis every hour or every half-hour.

Remarks.—The alkalies, generally, are solvents of false membrane, and lime-water is one of the pleasantest and most manageable for internal use. Eggert regards carbonate of potassa as almost a specific. J. D. Griscom, of London, gives the iodide of potassium. J. Gottstein, of Breslau, speaks of using repeated injections of lime-water into the nares in diphtheria. I do not know whether the lime-water which I administer acts through the circulation directly upon the inflamed surface, or whether its action is restricted to the epiglottis and the parts above; at any rate, I would not be willing to relinquish it, except, perhaps, for its equivalent in the carbonate of potassa.

Proposition IX.—Allow the patient to inhale the vapor of slacking lime.

Remarks.—The credit of this last expedient is due, I believe, to Dr. Geiger, of Dayton, Ohio, who reports great success.

Dr. B. B. Wilson, of Pennsylvania, with the same remedy, saved two cases while *in articulo mortis.* Dr. Wilde, of this society, reports two terrible cases, which I saw with him, and which were saved by the lime inhalations and the bromide of potassium. The best method is to slack the lime in an open pail or tub. Care is requisite not to annoy, frighten, or scald the patient. Some children are so much pleased with the relief it affords, that they seek it voluntarily after the first experience; others oppose its use, as they do every thing else. The confidence and coöperation of the patient are important, since all excitement increases the circulation, and the necessity for greater quantities of oxygen to properly arterialize the blood. The simple vapor of water has been and is extensively used and recommended, and some have seriously questioned whether any thing but moist air reaches the larynx when we employ the slacking-lime. I am not alone in contending that lime is inhaled, and that its effect is superior to that of vapor alone. Even at the temperature of 212° F., a pint of water will hold in solution five and a half grains of lime, and this is sufficient to give an alkaline reaction to the vapor which arises therefrom. If the patient resist violently, so that its immediate inhalation is rendered impossible, lime may be freely slacked in the room to such an extent as to keep the air constantly moist. I have used lime-water atomized by the spray-producer, and believe it objectionable in this disease. It annoys the patient, and condenses frigidly upon the face and neck.

Proposition X.—Take equal parts of impure carbolic acid and glycerine. Pour upon a teaspoonful of this mixture, in an open basin, a pint of boiling water. Renew this every four hours, and allow the patient to inhale its vapor for a few minutes. Let the preparation stand in the room till renewed.

Remarks.—Lime-water and carbolic acid, though possessing opposite chemical properties, are both useful, and both at the same time, though not at the same instant. I have observed, while using carbolic acid in surgical practice, that, whenever venous blood was touched by it, it instantly became arterial. Lime-water dissolves the false membrane, to use a paradox, before it is formed, while it is also gently stimulating and alterative to the inflamed surface. Carbolic acid acts directly upon the nerves and vessels of the larynx, trachea, and bronchi, aiding in the oxygenation of the blood and in the exfoliation of the diphtheritic deposit. It is also a valuable disinfectant, and will destroy any diphtheritic poison that may be lurking about unawares.

PROPOSITION XI.—Give an enema of strong hop-tea, at least twice a day. If the child be costive, add to the first enema one or two teaspoonfuls of table-salt.

Remarks.—The injection is given to unload the vessels, and to give the diaphragm free play.

Hop-tea is chosen on account of its sedative and antispasmodic influence. I avoid cathartics because of their disturbing and debilitating effects. A simple cathartic dose of calomel—ten to twenty grains—has been highly recommended; and, though I would not ordinarily give it, I can see no great objection to its use. I have been told that the late Francis W. Johnston, of New York, would not treat a case without it. He never *repeated* the dose. Had not Rush seen some good effect from its use, he could hardly have said, "Calomel is as efficacious in croup as bark in fever."

PROPOSITION XII.—Use externally some gently stimulating and anodyne liniment. I prefer linimentum saponis, slightly ammoniated, ℥ ij; tinct. rad. aconiti, ʒ ss. Apply this with a camel's-hair pencil.

Remarks.—Do not bind the liniment to the neck with cloths of any kind; it is not only liable to vesicate, but it adds greatly to the discomfort of the little sufferer to have the neck in any way pressed upon, or restricted in its movements. On this account the weight of poultices makes them objectionable. Tincture of iodine, either simple or compound, may be substituted for the liniment. Turpentine is a good application. My friend Dr. W. W. Rees uses kerosene-oil and prefers it to every thing else. I have had no experience with it, but am favorably impressed with the recommendation. The popular application of snuff and tobacco I regard as dangerous. Blisters are intolerable.

PROPOSITION XIII.—Let the diet be meat, broths, and milk, or milk-punch and wine-whey. Give water *ad libitum.*

Remarks.—I only intend to indicate by this proposition that we should sustain the patient. As an inflammatory affection it is of slight extent, and all experience goes to show that powerful antiphlogistic remedies are of no avail. On the contrary, an element of asthenia is often manifest at an early stage.

The amount of stimulation and support necessary must of course be left to the judgment of the physician in each case. Children at the breast should generally depend upon their natural aliment, but even with them stimulation is sometimes necessary.

PROPOSITION XIV.—As a rule, I am opposed to topical applications.

Remarks.—Dupuytren, Trousseau, Guiet, Bouchut, Ware, Eben Watson, Gottstein, and many others have applied and recommend a solution of nitrate of silver, forty or sixty grains to the ounce of distilled water. Horace Green, of New York, was the first to insist upon its introduction into the larynx. It has little or no effect directly upon false membrane, and must, therefore, to do good, be applied early, that it may exert its alterative influence upon the inflamed surface. Many acids and alkalies have also been used with the probang—the acids hardening and shrinking, and the alkalies softening and dissolving the membranous deposit. Iodine, and the subsulphate and sesquichloride of iron, with their blackening and accumulating presence, have also been used; but I am happy in the belief that the inhalations of the vapors already alluded to render all these distressing expedients not only inexpedient, to use a paradox, but positively harmful.

Proposition XV.—Tracheotomy is unjustifiable, except as a *dernier ressort*, and even then it is generally a forlorn hope.

Remarks.—I do not doubt that lives have been saved by it, yet I strongly suspect, though I dare not assume, that an equal number have been destroyed by it. It has many advocates and defenders, while very few say any thing in its detraction. I shall, therefore, speak the more earnestly, because I know not who are to be my supporters. Jacobi says, "The omission of it is homicide." Some of its most enthusiastic friends are named among the following statistics of the operation, which are all that I have been able to lay my hand upon:

	Operation.	Recov.	Deaths.
Bretonneau, ...	20	6	14
Trousseau, ...	150	39	111
Lothar Voss, ...	43	9	34
Waldemar von Roth, ...	48	11	37
Krackowizer, ...	56	15	41
Jacobi, ...	62	13	49
Gilfillan, ...	3	2	1
George Buchanan (Glasgow), .	26	9	17
	408	104	304

Eberle says, "All experience has decided against tracheotomy."

Dickson has little faith in it, and Dr. Frank H. Hamilton no more than I, as he himself has told me.

Ryland says: "With regard to the general results of tracheotomy when performed for the cure of croup, I have no hesitation in saying that they are so unfavorable as to warrant us in the strongest condemnation of it, under almost every conceivable circumstance." Dr. Cheyne contends, and his argument applies to a certain proportion of cases, "that death does not occur because there is an insufficiency of air admitted into the lungs to effect the arterialization of the blood, for," he says, "three eighths of the aërial canal is always open, constituting a space quite sufficient for the transmission of all the air necessary to the maintenance of the process of respiration." Dr. Eben Watson, of the Glasgow Royal Infirmary, sums up his experience in the following words: "Tracheotomy should on no account be performed during the exudative stage of croup; for it is either useless in the worst cases, or positively hurtful in those where there is any hope of recovery." In an excellent article contributed by this Society to the Society of the State of New York in 1863, Dr. William Gilfillan says: "Tracheotomy is an operation in which every thing is to be gained and *nothing can be lost.*" The italics are his own. And yet in this very communication he enumerates "certain causes of death which are incident to the operation," and, of those causes of death which arise subsequent to the operation, "some," he says, "spring from the operation, and some are accidental."

I quote again: "Hæmorrhage during the operation may cause death, or greatly weaken the already depressed vital powers;" again, "Hæmorrhage into the trachea may cause obstruction of the respiration, or even suffocation." Of course he gives directions for avoiding these accidents as far as possible. Yet the fact, that they may occur, shows that the operation is one in which life may be jeopardized beyond the risks incident to the disease itself. Again, we are told that the proper introduction of the canula "is a nice point and only to be acquired by practice." Again: "It is a matter of vital importance that the tube inserted, and through which the patient respires, should be as large as possible. . . . Trousseau first drew attention to this point, that many persons died after tracheotomy, from the tube introduced being too small to permit air enough to enter the lungs for the oxygenation of the blood."

Another fact which Dr. Gilfillan records (and I do not question the accuracy of any of his statements) is, that "the lungs" are sometimes "affected by the direct admission of cold air, and pneumonia and bronchitis follow." He says also: "After every case of tracheotomy, there is, I think, more or less bronchitis when the patient survives the first few hours." "A more serious form of the disease may occur, or pneumonia may arise." After these dangers are past, he tells us, very truly, that our next solicitude will have reference to the removal of the tube. "If this is not done soon, the larynx from disuse contracts so as to be too small for respiration." He says (and who will question it?) that "it is a grievous affliction to be condemned to breathe through a canula for the term of one's natural life." I have known a case, Mr. President, in which every attempt to get rid of the tube was attended with such serious symptoms, that it was worn for twelve years, and then I lost sight of the patient.

On the other hand, it occasionally happens that the tube creates so much irritation that it has to be dispensed with from the first; in which case, unless we can keep the wound open by means of a simple wire dilator, such as Marshall Hall describes in *Braithwaite's Retrospect*, part xxxv., we can, of course, expect no benefit from the operation.

The advocates and champions of tracheotomy are constantly urging us to operate earlier, that we may have a larger percentage of recoveries. Now, to my mind, the only justification for this operation is in its use when all else has most signally failed. Even then, it is hardly safe to assume that the few cases which recover are saved by it, for no practitioner of experience can have failed to see cases recover without operation, after all hope had been relinquished. Indeed, I cannot help recognizing the probability that some of the fatal cases after operation would have recovered, but for the additional risk incurred by it.

Now, I have no doubt that, by resorting to tracheotomy at an earlier stage of the disease, we should have a greater apparent success. We should, of course, include a greater number of those cases which would recover if let alone; and a sufficient percentage of these would live in spite of both disease and doctor, to increase very considerably the proportion of successful operations. When the enthusiastic advocates of tracheotomy succeed in persuading the whole profession to operate early, we may be very proud of the record, as we read recoveries forty per cent. instead of twenty as now, but who shall answer for the other sixty, thirty of whom would have recovered but for rash interference?

Operate early? Why, Mr. President, there was never a more dangerous exhortation. You cannot *early* know that there is any necessity for such an act. It is admitted by the most zealous operators that death may occur as a direct consequence of tracheotomy—by syncope from hæmorrhage, by suffocation from hæmorrhage, by a tube too small, by irritation, by bronchitis, by pneumonia—and we may increase the list by reference to all those accidents which are occasioned by the struggling of the patient, and by the constant moving up and down of the part to be operated on (unless well secured by the hook). These accidents I need not dwell upon, nor need I claim that they are unavoidable; the œsophagus and the pleura have been opened in this way.

Again, is it not probable that many a child has died because of the greater quantity of oxygen required, while, under violent exercise and fright, striving to escape the operation?

In view of these undoubted facts, have we any right or reason to assume that the *one* out of *four* or *five*, who has lived through this ordeal, was saved by it? Indeed, have we not reason to suspect that, if all the cases which have been submitted to the knife had been mildly treated without it, we should have had from fifteen to twenty per cent. more of recoveries.

I close this paper, which has already far exceeded the limit which I prescribed to myself, with a report of all the fatal cases of croup which I have seen during the last three years:

Case I.—A little German boy, four years of age, living in Atlantic Street, became hoarse May 16, 1869, but was allowed to run about as usual, in and out of doors. May 17th, was so much worse that his parents vomited him profusely with ipecac. and purged him with oil. Steadily the disease advanced, till, on the 18th, Dr. Skene was called. He found the patient weak; pulse rapid; breathing short and laborious; countenance anxious; skin and lips slightly livid. He ordered, at once, the plan of treatment sketched in this paper. This treatment was rather inefficiently carried out till the child died, on the twenty-fourth day from the seizure.

I was in consultation from the second day, and this is the first case of croup which I have seen die in over three years. I report it, because it confirms my views of the proper treatment of croup, as certainly as did the recovery of all the other cases. A powerfully perturbing treatment was instituted here before the doctor was called. A nauseant and emetic was given, with all its prostrating effect; the lungs were filled with mucus, which the child had no power to throw off; the alimentary canal, from one end to the other, was irritated and excited to such peristaltic action as would necessarily disturb every natural function. The child grew rapidly worse after the emetic. The breathing was much relieved by the subsequent treatment, and *finally the death* was *more* from asthenia than apnœa. On the day before his death an accident occurred in the treatment, which should be mentioned, though I suppose it did not alter the result. The nurse gave a teaspoonful of turpentine instead of lime-water, and, being frightened, ran to the nearest apothecary, who directed a teaspoonful of sweet oil. Some irritation of the bladder and urethra was the consequence.

Case II.—Lizzie C., aged four years; hoarse several days; really no other symptom, except an occasional slight, croupy cough; bright, playful; appetite good; skin natural; tongue moist; patient but little disturbed at night. On the third day, toward evening, exacerbation of all the symptoms, with profuse coryza. Difficulty of breathing increased through the night, but was not very great till the morning of the fourth day. Dr. Mitchell saw her at 9 A. M., and advised twenty grains of calomel in addition to my usual treatment. Steadily the symptoms progressed. The amount of air which entered the chest was enough to sustain any child, if the processes of endosmose and exosmose had not been prevented by the condition of the mucous membrane in the air-cells; and yet no bronchitis was discoverable by any physical sign. She died, not from laryngeal obstruction, but from want of power in the lungs to absorb oxygen and emit carbonic-acid gas. There was no struggle during the last two hours, but a gradual sinking, such as you would expect, as the effect of carbonic acid upon the brain.

This was no ordinary case of croup, as the sequel showed, for six days later an aunt, in the same house, had a severe attack of diphtheria; then the grandmother, also in the same house, had acute laryngitis, losing her voice entirely; and in less than another week a cousin in the same house had well-marked scarlatina. They all recovered.

A fatal case of laryngo-tracheitis having occurred under my observation, since the reading of the above paper, I append it here:

I was invited on Tuesday, the 14th instant, to see a little daughter of Dr. Caldwell, who is an active member of this Society. The patient was three years of age, and of good constitution.

I learned that, for about ten days, she had suffered with subacute bronchitis, of not very severe character, yet sufficiently grave to have been under treatment much of the time. Had vomited several times, as the effect of ipecacuanha, and had taken as an expectorant a mixture of squills, ipecac., tolu, and ammonia. Castor-oil was given as a cathartic once or twice, and her chest was enveloped in oiled silk. At three o'clock on the morning of the 14th, symptoms of laryngitis declared themselves—croupous cough, labored perspiration, and marked aphonia. The expectorant mixture was now given, and occasioned free emesis. A dose of castor-oil was also administered, which operated late in the day. Flaxseed and mustard poultices were applied to the chest, and flannels wet with liniment about the neck. I first saw her ten hours after the attack. She was profusely catarrhal, and continued so all the afternoon. Both lungs were loaded with mucus, at times producing loud mucous *râles*. I recommended the plan of treatment which I have detailed to you, but had little faith in the efficacy of any plan that could be adopted. The preceding and accompanying bronchitis had called for and received a course of treatment which I believe to be injurious in croup. I am not criticising the course pursued; it was orthodox, and the croup could not be anticipated. Drs. Byrne, Whaley, and Shuttleworth, all saw the case, and made valuable suggestions, but gradually the little sufferer sank, apparently expanding sufficiently the chest, and receiving air enough, but for the wall of mucus between it and the blood.

I have not thought it worth while to report successful cases, because it is almost essential to *see* a case of croup in order to judge of its importance or significance. The following will suffice to illustrate a class of cases which I think are not only saved from suffocation, but are prevented from reaching that stage of deep distress with which we are all too familiar:

Harry H., Schermerhorn Street, November 25, 1869, was taken sick with feverishness and slightly croupy cough; grew worse in the night, and on the morning of the 26th was profusely vomited with syrup of ipecac.; but no improvement took place. At 11 A. M. I was sent for, and from that time until evening the dyspnœa was of that persistent character which marks the generally fatal case. I administered at once a ten-grain dose of the bromide of potassium, and put in requisition all the other means set forth in the above paper. A peck of lime was slacked lump by lump, in an open wash-bowl. Half a teaspoonful of lime-water was taken every half-hour. The carbolic acid and glycerin mixture (equal parts), one teaspoonful to half a pint of boiling water, also gave off its vapor every three hours, and at 9 o'clock P. M. we felt that the prospect for recovery was good. The *bromide* was continued in five-grain doses every six hours, and in all other respects the treatment, through that night and the next day, was as at first. The convalescence was gradual but uninterrupted.

ART. III.—*On the Origin of Diabetes, with some New Experiments regarding the Glycogenic Function of the Liver.*[1]

By W. T. LUSK, M. D., Professor of Physiology, Long Island Medical College.

WHEN Bernard had completed his lectures in the winter term of 1854–'55, few questions in physiology were apparently more completely settled than that of the glycogenic function of the liver. The consequences of the new doctrine were of the utmost importance, for, once settled that sugar is the product of animal as well as vegetable organisms, and that an organ can be found which is singly concerned in its production, we would have at once, in the exaggerated performance of a normal function, the proximate cause of diabetes. There had been, from the first, no lack of opposition to Bernard's views, but so skilfully had he defended his position that, at the time of which we make mention, the very objections of his critics had only served to increase, in the eyes of the candid, the magnitude of his triumph.

Schmidt, of Dorpat, had stated, in a memoir, published five years before, that the doctrine of the formation of sugar by the liver was inadmissible, because sugar was equally to be found in the blood from the jugular and portal veins. Sugar he regarded, therefore, rather as a product of the tissues, comparable to urea. But Schmidt's experiments showed that he had not fulfilled the physiological conditions requisite for success. Subsequently, after repeating them with the precautions indicated by Bernard, Schmidt hastened to acknowledge the correctness of the results obtained by that physiologist.[2]

Schmidt and Bernard had both agreed in considering sugar as a normal product of the animal organism; whereas it had previously been regarded as solely derived from the vegetable kingdom. Many naturally clung to the ancient belief, in spite of all testimony in favor of the truth of the new doctrine. Conspicuous in this class was M. Figuier, who saw no sense in the organism giving itself the trouble to manufacture a substance so easily obtainable from external sources. To the statement that sugar was found in the blood of carnivorous animals, Figuier replied that the carnivora feed upon the flesh of the herbivora only, whose blood and tissues contain glucose derived directly from the vegetable materials serving them as food. Now, even if we should admit, for argument's sake, that the flesh of herbivora really contains glucose, there would none the less remain for explanation the absence of sugar in the vessels of the portal circulation, and its marked presence in the hepatic veins. To this it was stated that, in the portal vein, the sugar was masked by the presence of albuminose; but Bernard demonstrated the presence of sugar in small quantities in the blood of the portal vein in animals fed upon a mixed diet, containing amylaceous and saccharine matters, notwithstanding the presence of albuminose.

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[1] Paper read before the Medical Society of the County of New York.

[2] "Propriétés et Altérations des Liquides de l'Organisme," vol. ii., p. 98, by Claude Bernard.

At a later period, after Bernard had isolated the substance from the liver, to which he gave the name of "glycogen," M. Sanson[1] substantially reiterated Figuier's doctrine, with the difference only, that now the presence of this new substance in the blood and tissues was substituted for the glucose of his predecessor. In this form the question was submitted by the Academy of Medicine of Paris to a special commission, composed of MM. Bouley, Poggiale, and Longet, who failed, however, to confirm the experiments upon which the pretensions of M. Sanson were based.

So far the victory of Bernard was complete. For the first serious blow dealt at the new doctrine we must turn to the very remarkable experiences of Dr. Pavy, first published by him in the "Philosophical Transactions" for 1860, and which have since become generally known through his work on "Diabetes." Dr. Pavy begins by fully admitting the accuracy of Bernard's experimental results. If he arrives at different conclusions, it is because he adopts a different method. What Bernard has proved, says Pavy, is true of dead but not of living animals. Therein lies the entire difference. The liver, he says, during life, never contains sugar; nor is there any difference in the sugar found in the hepatic veins during life, and the same trace that may be found elsewhere in the vessels of the body.

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[1] M. Sanson, sur "l'Origine du Sucre dans l'Economie Animale," Journal de Physiologie, vol. i., p. 244.

The following was the plan adopted by Bernard to demonstrate the formation of sugar by the liver: A dog, that had been kept carefully confined to a flesh-diet, was instantly killed by breaking up the medulla oblongata, by which method life is suddenly arrested in the full performance of all its functions. Through a small incision in the abdominal walls a ligature was placed around the vessels going to the liver. Then, opening the abdomen and thoracic cavity freely, ligatures were placed around the vena cava above and below the point of entrance of the hepatic veins. The precaution of first ligating the portal vessels through a small incision was necessary to prevent a reflux of blood from the liver, resulting from the altered conditions of pressure that take place when the abdominal and thoracic cavities are freely opened. When the operation is thus performed, the results are invariable. In the portal vein we detect, at most, a mere trace of sugar; while a clear solution, prepared from the blood of the hepatic veins, furnishes an abundant reduction of the copper and potassa test. So, too, a decoction of the liver itself displays the evidences of the presence of a large amount of sugar. Bernard has shown that the liver does not directly form the sugar thus obtained, but first manufactures glycogen, a substance resembling starch, and which, under the influence of certain ferments, is capable of conversion into sugar. Upon these, and a multitude of similar experiences, he bases the glycogenic function of the liver, and declares broadly that that organ is the single source of all the sugar found in the economy, except the trifling quantity that sometimes exists in the portal vessels after the ingestion of saccharine articles of food—a quantity, however, always inferior in amount to that contained in the blood that comes directly from the liver.

Pavy was led, by noticing, in some experiments, the extremely small amount of sugar in the blood obtained by catheterization from the right side of the heart during life, to examine the liver of an animal instantly after death, and discovered the surprising fact that, when measures were adopted to immediately arrest the conversion of the glycogenic matter in its tissue, the liver contained no sugar, or only a mere trace. The conclusion appeared obvious; Bernard had mistaken a *post-mortem* for a physiological production. Bernard had owed much of the success of his doctrines to the simplicity of the experiments upon which they were based. They were all easy of confirmation. But this additional one of Pavy possessed a like merit. It was widely repeated, and its accuracy established. Pavy examined the liver in animals just killed. Jaeger and Meissner went a step further, and removed a portion of the liver from living animals. They too arrived at the same result. The glycogenic function was apparently doomed. About a year ago, however, Dr. A. Flint, Jr., came to its rescue in a paper entitled "Experiments undertaken to reconcile some Discordant Observations upon the Glycogenic Function of the Liver."[1] In this paper he proposed an ingenious solution of the difficulty, to wit: "That during life the liver contains only glycogenic matter, and no sugar, because the great mass of blood which constantly passes through that organ washes out the sugar as fast as it is formed; but after death, or when the circulation is interfered with, the transformation of glycogenic matter into sugar goes on; the sugar is not removed under these conditions, and can then be detected in the liver." To demonstrate this view, Prof. Flint first verified the absence of sugar in the liver removed rapidly from a living animal, and then showed its presence in considerable quantities in the blood of the hepatic veins after killing the animal and tying the vena cava above and below these vessels. The performance of the latter step of the operation was accomplished in about sixty seconds. Pavy[2] replied to this, "That so quickly is sugar formed in the liver after death, that the result was to have been expected, considering the expenditure of time involved in applying the ligatures."

The following experiments were undertaken to test the validity of the objection so strenuously insisted upon by Dr.

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[1] New York Medical Journal, January, 1868.

[2] New York Medical Journal, September, 1869, p. 626.

Pavy. All the experiments were made upon dogs. I first confirmed, as many had done before, the statement of Pavy, that the liver, if removed rapidly from an animal after breaking up the medulla oblongata, and treated by the methods recommended to arrest fermentation, is free from sugar. The question then suggested itself, Why not obtain blood in the same way directly from the hepatic veins immediately after killing an animal, without stopping to apply the ligatures? For a short time, after breaking up the medulla, the circulation continues undisturbed, and blood is easily obtained as it comes from the liver, by simply elevating that organ, and opening the hepatic veins as they emerge beneath. The act is quickly performed, certainly requiring no more time than to remove a portion of the liver, cut it in pieces, and transfer it to boiling water; but blood, thus obtained, always presents a marked saccharine reaction. This plan, however, would not stand the crucial test; for, when, after obtaining the blood, I quickly removed a portion of the liver also, the latter was found to already present the changes ascribed by Pavy to *post-mortem* fermentation. The time required for the twofold operation was sufficient for a saccharine reaction to be inaugurated.

In accordance with Dr. Pavy's recommendation, I next resorted to his plan for obtaining blood from the right side of the heart, in what he considers its natural condition. This consists in breaking up the medulla oblongata, opening the chest, seizing the heart and placing a ligature around its base, when, according to Dr. Pavy, the blood contained in all its cavities will present the conditions belonging to life. The blood in the right cavity, and specimens procured from the jugular and portal veins, when compared together, he says, present no special differences in their behavior. I soon abandoned this method, however, on account of the smallness of the quantity of blood that can be obtained from a single side of the heart in a dog of ordinary proportions. Indeed, unless the amount of sugar in the right side of the heart was something considerable, which was hardly to be expected in view of the large dilution of the hepatic blood with the non-saccharine fluids of the venæ cavæ, any very appreciable difference between a thimbleful of blood from this source, and a corresponding quantity from other parts of the body, was hardly to be predicated.

In view of this objection, I therefore resolved to follow the directions of Bernard for obtaining blood directly from the right side of the heart in the living animal, by passing a catheter down into its cavity, through the medium of the jugular vein. I felt the more inclined toward this plan because of the opposition in the results arrived at, respectively by Bernard and Pavy, in comparing blood, obtained in this way, with specimens derived from other sources. Thus we read in Dr. Pavy's work on "Diabetes" (p. 49): "Bernard records an experiment made before his class, in which the blood removed from the right side of the living heart gave a neat reaction with the copper solution, while the blood of the carotid artery and jugular vein gave no reaction at all." On the other hand, Dr. Pavy's own experiments on living animals, he thinks, show that sugar does not exist "to a larger extent in the right side of the heart, than to that which has hitherto been recognized as belonging to the blood of the arterial and the general venous systems; and that, should sugar appear to any amount in the right ventricular blood, it will also be found to a corresponding extent in that of the carotid artery and the jugular vein." A certain amount of perplexity is natural, when we find two eminent observers at variance about such a thing as a fact. Bernard's result was certainly no accident. If we turn to the passage in the "*Leçons de Physiologie Expérimentale*," referred to by Dr. Pavy, we find that Bernard is simply repeating before his class an experiment, with the results of which he is already obviously familiar. Dr. Pavy suggests that "a fallacy may have arisen from the animal not having been in a tranquil state," or because, to quote his own words, "nothing was formerly known about the differences I have pointed out, that are observable in the blood under different conditions." We look for these conditions, and find them given as follows: "Should there be any extensive amount of muscular disturbance, or embarrassment of the breathing, a considerable indication of the presence of sugar is sure to be met with."

The first of these facts was one with which Bernard was certainly familiar. In the lectures already referred to (v., p. 267), he states explicitly: "We know that we have only to cause animals to make violent muscular movements, especially those of the diaphragmatic and abdominal muscles, to find sugar in the jugular vein." As to the results arising from simple embarrassment of breathing, the experiments of Bernard and Pavy do not appear to be exactly in accord. Bernard allowed dogs and rabbits to perish in a confined space, but never found them to become diabetic (p. 349), *Leçons de Physiologie Expérimentale.* Pavy, on the contrary, affirms that, "by simply interfering with the proper performance of the breathing, he determined such a presence of sugar in the circulation as to have rapidly occasioned a strongly saccharine state of the urine." This result he attributes, and probably correctly, to the mechanical compression of the liver from violent contractions of the muscles of the abdominal parietes; so that the conditions mentioned by Pavy are reduced to the single one of "muscular disturbance," from whatever cause arising. Now, we are puzzled to understand how this source of fallacy could have possibly invalidated the experiments of Bernard. Bernard, it will be remembered, found sugar in the blood of the right side of the heart, but none in that of the jugular vein; whereas the source of fallacy pointed out by Dr. Pavy would inevitably have led to the presence of sugar in that vessel, as, to once more quote his own language, "should sugar appear to any amount in the right ventricular blood, it will also be found to a corresponding extent in that of the carotid artery, and the jugular vein." It therefore seemed desirable that experiments should be undertaken to determine the precise truth regarding these opposing statements.

I may mention, however, at this place, that I began my labors, in connection with this subject, with a strong predilection in favor of the views entertained by Dr. Pavy, and it was with the intention of confirming them, if found compatible with fact, that every step in my earlier experiments was undertaken. I naturally, therefore, employed Dr. Pavy's methods, and endeavored to avoid the sources of error indicated by him. To prevent the disturbances arising from the operation itself, I adopted in many instances the suggestion of Prof. Flint, which consists in first exposing the jugular vein, and then, after placing a loose ligature around it, to allow the animal three or four hours of repose before proceeding to catheterization, a period certainly sufficient to admit of a return to the customary conditions of the tranquil state. With a little practice, the passage of the catheter through the jugular vein into the heart is performed so quickly, and with so little apparent distress on the part of the animal, that the blood, which escapes by jets when the heart is reached, may fairly be considered as in a physiological condition. As the quality of the blood is known to change when considerable quantities are withdrawn at one time, I restricted the amount taken to about one fluidounce.

Again, in other cases, in order to control my results, I proceeded to catheterize immediately after exposure of the vein. The result was always the same. The blood obtained was treated with alcohol, as directed by Pavy. The clot remaining after filtration was thoroughly washed out by repeatedly transferring it to a small mortar and thoroughly incorporating it with fresh quantities of alcohol, until the filtered fluid no longer gave a trace of saccharine reaction. The alcoholic solution thus obtained was evaporated to a small bulk, and the quantity of sugar estimated by both Fehling's test-solution and Pavy's modification of the same. It proved to be very small indeed, and averaged, in a fluidounce of blood, from a quarter to a half a grain, the latter amount in cases where the animal at the time was in full digestion. The results did not materially differ from the careful estimates of Pavy, and certainly were in striking contrast with those obtained from the blood taken from the same source after death.

It next followed to ascertain if a similar trace existed normally in the jugular vein. Bernard's experiments had been negative, except during the period of digestion. I therefore employed dogs that had been kept fasting twenty-four hours. In these cases every precaution was taken to avoid disturbing the tranquillity of the animals. The jugular vein was first exposed, and the animals allowed several hours of repose. No disturbance ensued when the jugular vein was divided with the scissors, the animals remaining perfectly quiet. The quantity of blood collected was restricted to one ounce. But in every case a feeble reaction with Fehling's test was manifested. Pavy was right in maintaining that traces of sugar are found in different parts of the venous system.

The idea suggested itself that this slight trace of sugar was due to some change that took place in the blood after its removal from the body. M. Sanson[1] had stated with great positiveness that, while blood freshly drawn from an animal contains no trace of glucose, a very decided saccharine reaction is obtained if the same blood is exposed in a vessel to the air at a moderate temperature, the extent being determined by the length of time allowed to elapse previous to examination. I therefore took three portions of the same specimen of blood, obtained from a dog confined to a flesh-diet, and subjected to a rigorous comparison the blood freshly drawn, and that which had been exposed for twenty-four and forty-eight hours, under the conditions assigned. The amount of sugar contained in all the three specimens was exactly the same.

There remained, then, but one question to settle, but that was the heart of the controversy. It was requisite to determine the relative quantities of sugar in the blood of the same animal when drawn from the right side of the heart, and when taken from other parts of the body. Was it, indeed, absolutely true that the traces of sugar found in all parts of the body are really precisely the same in amount? In answer to this question, I confined myself to comparisons instituted exclusively with the blood of the jugular vein. At first I endeavored to estimate the precise quantity of sugar in a given specimen by means of the familiar decolorizing action of sugar upon Fehling's standard test-solution. But it proved, upon actual experiment, that, however valuable the volumetric method may be for ordinary purposes, it cannot be implicitly relied upon to fix exactly the minute quantities of sugar contained in small specimens of blood. It is, in fact, always a nice point of observation to determine the precise moment when the color is fully discharged from the test-fluid; and, when we are dealing with mere fractional parts of a grain, the result is pretty sure to correspond to our previous conceptions. It is almost impossible for our prejudices not to interfere with our judgment.

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[1] "De l'Origine du Sucre, etc." Journal de la Physiologie, 1856, p. 244.

But, for the determination of the problem, a process to exactly estimate the quantity of sugar in a given portion of blood was unnecessary. It was simply requisite to ascertain whether the blood from the right side of the heart contains sugar in greater abundance than is found elsewhere. With a view of obtaining some suitable method to settle this point, I made a large number of experiments, and finally adopted the following plan: A fluidounce of each of the two specimens to be compared was carefully measured, and to these respectively was suddenly added double their bulk of proof-alcohol. To obtain a clearer solution, they were then allowed to stand for a few minutes to permit the coagulated portion and blood-corpuscles to settle to the bottom of the vessel. They were next thrown upon filters, and the alcoholic extract allowed to drain off. The clot was further subjected to frequent washings with alcohol, until freed as far as possible from all traces of saccharine matter. The combined fluids obtained by filtration were then evaporated over a water-bath. As, when reduced to a small bulk, the fluid became dark-colored, thick, and turbid, no matter how clear it may have been at the outset, I pushed the evaporation to dryness, and subsequently made a watery extract of the residue. For this purpose a given quantity of water, usually about two hundred and fifty fluid-grains, was rubbed with the residue in a mortar, and the whole then thrown upon a moistened filter. When this is done, a milky fluid passes through, which contains very nearly, if not quite, all the sugar contained in the specimen, together with a portion of the alkaline bases of the blood. Enough water was then added to make of each specimen exactly two hundred and fifty fluid-grains, with which to make our comparisons. I now found that if I took two test-tubes, containing each twenty grains of Fehling's solution, carefully diluted so as to make exactly one hundred fluid-grains, there was a marked difference to the eye in the degree of the reaction produced by the addition of equal quantities of the saccharine extracts obtained from the blood of the heart and the jugular vein. To make the matter sure, I then carefully filled a burette with the solution from the right side of the heart, and added it slowly to a known quantity of Fehling's solution in a test-tube kept at the boiling-point. When the reaction had become very distinct, at a point just short of completely discharging the color, I stopped and ascertained the quantity of saccharine fluid employed. Then taking an equal quantity of the solution prepared from the jugular blood, I proceeded to test it with precisely the same quantity of the test-fluid. The increase of the reaction in favor of the blood from the heart was very striking. Indeed, while the reduction of the copper in the latter case was nearly complete, in the blood from the jugular vein the reaction was scarcely perceptible until the liquid had cooled. After letting the two specimens stand for twenty-four hours, a very decided difference still remained.

With a view of still further avoiding error, I repeated the process with two specimens prepared from the same blood, and found that, neither in regard to the vivacity of the reaction, nor in the amount of the precipitate, was there any palpable difference to be remarked. The method, therefore, might be regarded as adequate to the determination of the question proposed. The difference remarked in the first case was not accidental, but it became further necessary to determine as to the constancy of the result. I therefore repeated the process described, in four additional instances. Twice the dogs had been kept without food for upward of twenty-four hours; twice the blood was withdrawn while the animals were in full digestion. So far as the eye could determine, by the quantity of copper reduced, the blood from the right side of the heart not only showed an excess, but contained from two to four times as much sugar as that from the jugular vein. In three of the instances the blood was first withdrawn from the heart, and afterward from the jugular vein. As Dr. Pavy has affirmed that, "should sugar appear to any amount in the right ventricular blood, it will also be found to a corresponding extent in that of the carotid artery and jugular vein," the objection cannot be made that the excess of sugar in the blood of the heart is due to the disturbing effects of the operative procedure. Indeed, the preponderance of sugar should have been in favor of the specimen last drawn, were the assertion perfectly correct.

Now, fully admitting the truth of Dr. Pavy's discovery concerning the *post-mortem* nature of the abundant sugar-production by the liver as noticed by Bernard, we are authorized, in view of the foregoing experiments, in adopting the following propositions:

1. That the blood of the general system, in carnivorous animals confined to a nitrogenous diet, contains appreciable quantities of glucose, not only during the period of digestion, as admitted by Bernard, but even in cases where animals have been deprived of food for a considerable period of time.

2. That the blood of the right side of the heart contains from a quarter to half a grain of glucose per fluidounce, under strictly physiological conditions.

3. That the quantity of glucose in the right side of the heart is from two to four times greater than that found under corresponding circumstances in the jugular vein.

4. That this excess argues a by-no-means insignificant amount of sugar in the pure hepatic blood, before it has become largely diluted with the comparatively non-saccharine fluids of the venæ cavæ.

5. That we are forced to admit the fact of sugar-formation by the liver, though we fail to detect the presence of sugar in the liver-tissue, when, after death, the fermentation of the glycogenic matter is prevented.

Now, are we, in view of all the facts, justified in further assuming that the liver is the sole source of the sugar found in the economy? The question is of the utmost practical importance, as involving the origin of diabetes. Tiffenbach[1] employed the volumetric method in the examination of blood from the right side of the heart, the jugular vein, and the vena cava below the entrance of the hepatic veins, and found the sugar diminished in quantity in the order of the specimens given. He therefore concluded that the farther from the heart blood is taken, the less sugar it contains. We have already noticed the liability to error in all attempts to fix with accuracy small quantities of sugar by the volumetric method; but, even if the results be positively correct, they do not absolutely exclude the origin of sugar from other sources than the liver. We need to be careful upon this point. Bernard proclaimed a new function for the liver, that of the secretion of sugar; but the liver had already one function, that of the secretion of bile. To meet this difficulty, M. Robin[1] declared that the liver consisted of two distinct organs—the one bile-producing, consisting of certain gland-like appendages to the bile-ducts; and the other glycogenic, composed of the lobules of the liver proper. Recent investigations, however, show that the bile-ducts originate in an interlacing net-work within the lobules and immediately around the liver-cells; so that we are confronted with the difficulty of two distinct secretions formed by the same elements of the same organ. Nor is this difficulty the only one. According to Meissner,[2] while the flesh of the mammalia contains hardly a trace of urea, it is found in the substance of the liver in very considerable quantities. In birds, in which uric acid takes the place of urea, the livers were found to contain many hundred times more uric acid than an equal weight of muscular tissue. Meissner, therefore, affirms that his experiments authorize him in concluding that the liver is the chief, if not the only, source of urea, in the mammalia, and of uric acid in birds; so that, if we make no distinction between the acts an organ accomplishes and its special functions, we find that another new function for the liver looms up before us. There is, however, a very suggestive resemblance in the conditions that give rise to an exaggeration of the quantity of both sugar and urea in the economy. In diabetes, according to Bouchardat, there is an excess of urea as well as of sugar in the urine. Both sugar and urea are increased in the blood, after violent exercise and during digestion. When the excretion of urea in excess takes place habitually, traces of sugar are usually found in the urine. In conditions of health, the quantity of urea in the blood is so small that its presence, even, was not detected until, by extirpation of the kidneys, a method was discovered to cause its accumulation in that fluid. We labor under a certain disadvantage in not being able to produce at will a similar accumulation of sugar in the blood by the removal of special organs; but Bouchardat[1] claims that a certain dependence exists between sugar-destruction and the normal action of the respiratory organs. Where there is, from any cause, an excess of glucose in the blood, every time the respiratory energy is diminished, either as the result of bronchitis or depression in the pulmonary action, glucose makes its appearance, or increases in the urine. It is at a period of life when the active capacity of the lungs is diminished, or the phenomena of respiratory combustion are on the decline, that glycosuria is most frequently manifested.[2]

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[1] V. Bericht über die Fortschritte der Physiologie im Jahre, 1868. Von Dr. G. Meissner, S. 209.

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[1] *Vide* "Histological Doctrines of M. Robin," New York Medical Journal, vol. iv., p. 412.

[2] Zeitschrift für Rationelle Medicin. Bericht über die Fortschritte der Physiologie, 1868, S. 213.

While, after birth, glycogenic matter is found in the liver only, it exists in the muscles and lungs during fœtal life. Bernard, having found a glycogenic matter in the appendages of the fœtus, supposed that they were temporarily charged with the glycogenic function previous to the development of the liver. Sometimes, as in the rodentia, this function is discharged by the placenta; again, in the ruminants, by the amnion; and in birds, by the walls of the villetine sac.[3] Rouget maintains that there is the same common source for glycogen in the tissues and in the appendages of the fœtus, and that its presence in one tissue does not depend upon its generation by another. In very young embryos, in the ruminantia, the amyloid substance disappears from the amnion and placenta, whereas it persists in the other tissues pretty nearly up to the time of birth. He insists "that the existence of an amylaceous matter does not indicate a new function of an organ, but a new property of tissue; the production of sugar is not the end, but simply the consequence, of the presence in the organism of zoamyline (glycogenic matter). The sugar, that urinary secretion causes to accumulate in the allantoid and amniotic tissues of the fœtus, whose tissues contain zoamyline, is the result of the destructive assimilation of this substance, as urea is that of proteine substances."[1] At birth the glycogenic matter disappears from the muscles, while it continues to form in the liver throughout the entire period of existence; but the glycogenic matter may reappear, at any time, in the muscles, when the conditions are such that reparative matter accumulates in tissues the contractile elements of which do not exercise their functions; as in cases of hybernating animals, and limbs paralyzed by division of the motor nerves.

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[1] "Sur une nouvelle fonction du Placenta." Journal de la Physiologie, vol. ii., p. 31.

[2] "Annuaire de Thérapeutique," Art. "Étiologie de la Glycosurie."

[3] Dr. A. H. Smith tells me that, in two cases of diabetes, he has witnessed a total disappearance of sugar from the urine as the result of the administration of oxygen.

There are many instances of diabetes in which some other mechanism than that of the liver seems requisite to satisfactorily account for its origin. We find there is nothing incompatible with our knowledge of the subject, in the supposition that all the principal tissues of the body may together or singly contribute, in certain states of disease, to swell the amount of sugar in the circulation. There is a simplicity about the hepatic theory of diabetes which is extremely seductive, but which, at the same time, affords just ground for suspicion. Until we know more about the processes of nutrition than at present, we must content ourselves with glimpses of truth, and not be too hasty in proclaiming that we have found the truth itself.

This much said in the study of the pathology of diabetes, the importance of the liver is not to be under-estimated. It is certainly, under normal conditions, the principal source of sugar in the economy. When Bernard found in examining the bodies of diabetics that, in some cases, the liver was augmented in size and gorged with blood, he was, perhaps, right in attributing to that condition an exaggerated sugar-production. But Tcherinow[2] recently reported a case of diabetes in which there was atrophy of the liver-cells, with destruction, consequently, of all that could have made the liver a secreting organ. Here Tcherinow attributed the diabetes to the condition of the liver, which no longer acted as a barrier, storing up in its substance the saccharine matters brought to it by the portal vein, and converting them into glycogen, but allowed them to pass through into the general current unchanged.

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[1] Rouget, "Des Substances Amyloides." Op. cit., vol. ii., pp. 83, 308.

[2] Zur Lehre der Diabetes Mellitus. Virchow, Archiv, 47 Bd., l. h., S. 102.

Bouchardat considers the exciting causes of the disease to be the abuse of starchy articles of food, and prolonged stomach-digestion, primarily due to eating too much and too fast. In such cases he says the gastric juice becomes so altered as to be capable of accomplishing the conversion of starch into sugar, a process normally effected by the pancreatic juice. As absorption takes place in the stomach, glucose is at once transmitted to the blood in great quantities, because the liver, through which it passes, is already saturated with glycogenic matter, as the consequence of a prolonged amylaceous diet. A small atrophied pancreas is not uncommon in diabetes. In a recent *post-mortem* examination of a diabetic patient, the only lesion I could discover consisted in total calcareous degeneration of the pancreas.

In a pretty large class, the primary cause of the disease is connected with disorders of the nerve-centres. Violent emotions, injuries of the head, and sexual excesses, have long been recognized as causes of diabetes. Quite recently, Dickinson[1] has found certain pathological lesions of the brain, which he regards as the cause, but which certainly read more like the results, of the disease. The nervous system can only act through its influence on nutrition, and may serve either as a stimulus to excessive production, or, by exercising a depressing effect upon functions, lead to insufficient destruction of sugar in the economy.

To sum up, the causes of diabetes are various, and the source of the sugar probably not confined to a single organ. At the same time the liver is to be regarded as the most active agent in sugar-production. At times it may be the indirect cause of diabetes by its failure to fulfil its function of arresting the passage of saccharine principles through it. We are to regard the labors of Bernard, Pavy, and Bouchardat, as severally adding to the sum of our knowledge of diabetes. There is no reason to suppose that the truth lies in any one exclusive theory. The problem is unquestionably a complicated one; and we must be content to get our knowledge piecemeal, until the time shall come when we shall fairly hold in our hands the key to the mystery of nutrition.

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[1] *Vide* Lancet, February 19, 1870, p. 268.

Art. IV.—*The Bromides; their Physiological Effects and Therapeutic Uses.*[1] By Z. C. McElroy, M. D., Zanesville, Ohio, President Muskingum County Medical Society.

The methods by which the action and uses of therapeutic agents have been determined, pursued from the remotest antiquity, have not yielded results at all commensurate with the skill and labor bestowed upon them. They were in the beginning individual empirical experiences, and they remain so to the present hour. As facts, they are treasures most precious. But their reapplication, in the absence of any general principles explanatory of their relations to the living organism, still depends on memory and individual judgment. The human body has been most patiently, industriously, and minutely examined and studied, in health and disease, but no broad and comprehensive generalization, or basis of life, throwing light on all its processes, in both conditions, is taught, either in schools or literature. The stately systems of nosology which reigned over the medical mind so many centuries, still leave their impress on the nomenclature and philosophy of health and disease of the present. The classifications of therapeutic agents have undergone no change whatever to make them correspond with the advanced and changed condition of philosophy, pathology, and organic dynamics. The novitiate in medical science, when he approaches the study of special therapeutics, does so almost unavoidably impressed with the idea of the separate entity of disease, and the local operation of remedial agencies. All through medical literature this is more or less positively inculcated. The separate entity of disease necessarily includes the idea of its local character, and writers on therapeutics teach, if they teach any thing, the local operation of remedial agents, and the causation of disease.

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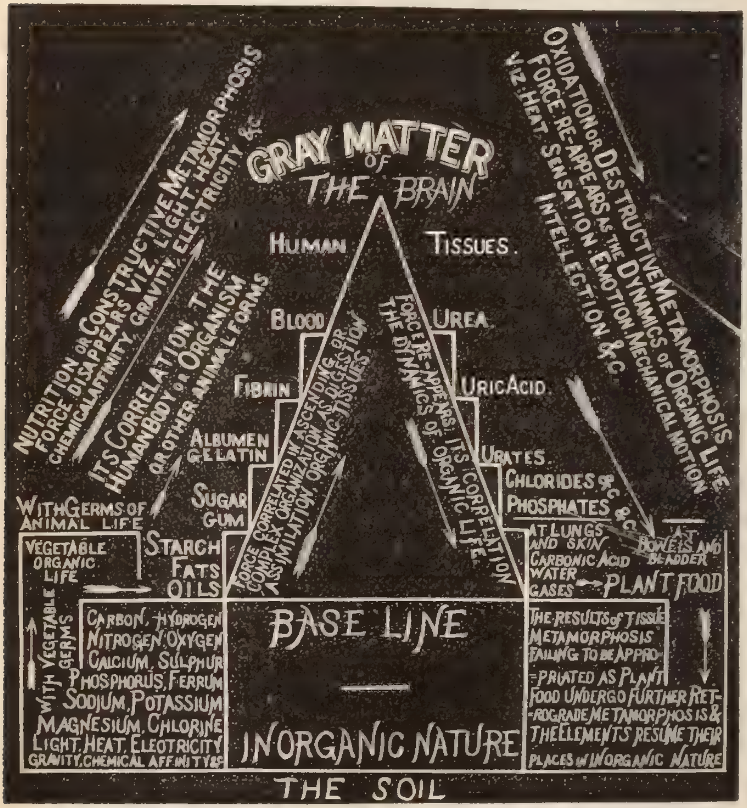
[1] Valedictory Address, 1869.

For these reasons, in this memoir on the "Bromides, their Physiological Effects and Therapeutic Uses," the beaten paths of the past will not be followed.

All of the processes of the living body will be reduced to the two generic totals of *nutrition and oxidation*, or *constructive and destructive metamorphosis.* Disease will be regarded as modifications of these two processes, and therefore generic and total; that is, at all times involving the whole organism. Therapeutic agents will, in like manner, be regarded, not as influencing a local part of the system, but these generic totals, by "promoting or retarding" them.

Negatively, it would seem that such a statement, as that all the processes of the living organism can be reduced to the two processes of constructive and destructive metamorphosis, would not need proof—would be self-evident. Yet, as taught in schools and literature, physiology and pathology do not do so. And, so long as the functions of the separate organs are studied with reference to their *secretions and excretions*, to the neglect of their positions in, and relation to, the living organism in its generic totals of nutrition and oxidation, the student of physiology will have his attention more or less diverted from the main purpose of physiology. No objection ought to be made to the study of the structure and purpose of the various organs, tissues, and textures, with all the aids from optics, chemistry, and comparative anatomy; nor to the study, chemically and microscopically, of their secretions and excretions—that is, the peculiar chemical condition of the organic matter which it is their apparent function to elaborate; but all these investigations should be subordinate to the leading one, of the relations they bear to the generic totals of ascending and descending metamorphosis.

The following diagram illustrates an ideal conception of these processes: For a base line, the condition of the elements in inorganic nature is taken. These elements are with a vegetable germ capable of forming—1. Vegetable organic compounds. 2. These, in turn, capable of assimilation, by the nutritive process, to the types and forms of animal organic life. Then set up other lines to form a triangle, with well-defined ascending and descending steps. On the base-line to the left, arrange the elements, carbon, hydrogen, oxygen, nitrogen, sulphur, phosphorus, calcium, soda, potassa, etc. Above the base-line place vegetable organic life. On the steps of the ascending line, to the left, place the different foods, as they ascend in complexity, and this represents constructive metamorphosis. On the first, or lowest step, place fat, oil, starch, water, and oxygen. On the second, the next in complexity of organic compounds, as gum and sugar, etc. Third step, albumen, gelatine, etc. Fourth, fibrin. Fifth, blood. At the apex, human tissues. The brain, of all the tissues, having the highest complexity, and in virtue of its importance in the



dynamics of organic life, may very properly occupy the most elevated position in the diagram. On the right line, or descending line, representing destructive metamorphosis, may be arranged the results of tissue-metamorphosis, or the descending compounds, the temporary condition of organic matter on its return to the position of its elements in the inorganic world. That having the highest complexity, as urea, may occupy the highest step. On the succeeding steps place the results of retrograde metamorphosis, in the order of their complexity, until the base-line is reached. Above this place carbonic acid, water, and gases, and the other results of tissue-metamorphosis, constituting plant-food. Failing to be so appropriated, the compounds break up, and the elements take their places again in the inorganic world.

In these transformations, nothing has been added and nothing lost; but, between the points at which the elements leave and arrive again at their positions in the inorganic world, all the varied phenomena of plant, animal, and intellectual life have been manifested. In the ascending or constructive metamorphosis, force steadily disappears, and its correlant is the organization itself, until the highest point is reached in the gray matter of the brain. Coincident with decay, or destructive metamorphosis, force, and all its varied correlatives in the dynamics of organic life, reappear, giving rise to all the chemical, thermal, sensory, mechanical, emotional, and psychological phenomena of human life.

In affixing the position of the various organic instrumentalities in this strongly-marked outline of the generic totals of the human organism, some are found to perform only one office, as the alimentary tract and ductless glands—assimilation only. Others, as the kidneys, elimination only; while others, as the heart, capillaries, liver, and lungs, perform parts of both acts; the skin and lower bowels apparently performing, vicariously, one or the other office, but mainly that of elimination.

In its dynamic aspects, the cerebrum, cerebellum, medulla oblongata, and spinal cord, are to be regarded as the main or central power-producers; the ganglions and plexuses—the so-called sympathetic system—as supplemental to the central masses; as from their anatomical arrangement and known physiological functions, each organ being supplied with a plexus. or ganglion, of magnitude corresponding with its importance, they are apparently intended to supplement, or isolate, and to some extent render each organ independent of the central power-producing masses in the distribution of dynamic capabilities: a very necessary arrangement, as otherwise accidental concussions, shocks, falls, etc., would more frequently prove fatal to life. While each organic instrumentality is a necessary fraction of the integer, or generic total, each organ has, to some extent, a separate and independent existence in virtue of these supplemental dynamic masses.

The nerve-cords proper are to be regarded as merely conveyers of sensation and dynamic force to and from the interior and exterior, and to and from all other parts of the organism; flashing, as it were, instantly, impressions and force all over the body.

In the past, and with a large proportion of my contemporaries, an *imaginary vital force* has been supposed to play a conspicuous part in organic life. The recent achievements of organic chemistry, and more exact views of the forces of organic and inorganic natures, have lifted the veil which has so long shrouded this so-called *vital force* in mystery. Organic chemistry now constructs, from inorganic elements directly, without the intervention of a germ, many of the less complex compounds of vegetable organization. Oils, fats, acetic and oxalic acids, have been thus constructed; but, though starch is almost within reach, it has never, as yet, been actually built up. The transformation of starch into glucose is an achievement common to manufacturers. Neither albumen, fibrin, nor organic tissue, has ever been made by organic chemistry, though the retrograde metamorphosis of animal tissue has been so controlled as to produce the same compound as its decay or destructive metamorphosis yields within the living body, as urea, uric acid, etc.

As formless organic matter, viz., oils, fats, etc., have been constructed from the elements directly, and as human tissues, composed of inorganic elements, are built up by the ordinary forces of inorganic nature, it is proposed to call this correlation of the physical forces the *formless force* or *organizing force* of organic life. The only features of organic tissues unaccounted for by the operations of the ordinary physical forces of inorganic nature are their *forms.* It is proposed to call that force which gives and preserves the types and forms of organic life the *form-force* or *architect of organization.* Its only office is the giving and preserving form, with the momentarily changing material of animal life. And, apparently, this form-force is what has hitherto been regarded as the *vital*, for all else in animal life can be accounted for by the operations of the physical forces of inorganic nature. Instead of occupying the most important position in animal life, as heretofore supposed, in the construction of organic tissues, its position is subordinate to the ordinary physical forces, as evidenced by pathological anatomy: for, if the types and forms of organized tissues were always reproduced in full dynamic integrity and form, there could be neither disease nor death from disease—animal life would be perpetual. Pathological anatomy is an account of what occurs when the *form-force* (so-called vital) fails to preserve form, though tissue is still constructed by the *formless force* without the normal form. The *formless* or *organizing* force is the laborer, building up; the *form-force, the architect*, giving and preserving form. Over the *formless* or *organizing force* therapeutic agents have some control, but the *form-force* is beyond their influence. In therapeutical discussion it must, therefore, be left out entirely.

It seems even probable that the form-force is not peculiar to organic life, but is a correlation of the ordinary physical forces of the inorganic world. The forms assumed by inorganic matter, in passing from the liquid to solid states or forms, are too constant and definite not to be under the control of an invariable form-force. And it may hereafter be determined that the so-called vital or form-force of organic life is but the correlative of the form-force of the inorganic world. Loss of type, the organizing or formless force continuing in activity, tumors and other morbid growths, foreign in their structure and type to the human body, are produced, which, generally, are remediable only by their removal by surgical proceedings. As the inherent stability of some of them in organic nature is inferior to that of normal tissue, some therapeutic agents seem to possess the power to hasten the disintegration of some forms of them, even to the extent of their entire removal, among which the bromides and iodides are conspicuous.

A few more points in this physical basis of life remain to be considered, in completing the data for determining the physiological effects and therapeutic uses of any remedial agent. Among these are conceptions of the process of organic tissue-making, from formless organic compounds—histogeny; and the evolution of force.

Authorities for a few years past have generally conceded that solid tissue was constructed by means of minute cells, nourished by the highest organized constituents of the blood—fibrin. But the latest authority (Beale) is to the effect that it is by means of minute organic crystals, or granules, that the solids are formed. Not seeking to establish either of these minute points of histogeny in this memoir, the idea of crystals, or granules, is adopted, as more in accordance and harmony with the known phenomena elsewhere in Nature, of the formation of solids from fluids, and because few of the premises or conclusions of this essay rest on authority.

Time, mode, and velocity, are all important elements in the process of the formation of solid organized tissue from formless organic materials. The moment of time at which the conversion takes place is, perhaps, the most important in a therapeutical point of view; for it seems probable, if not certain, that it is then and there that most, if not all, therapeutic agents exercise their influence, for good or evil, in the living body. The negative of this proposition would seem to be, in itself, sufficient evidence of its correctness; for, so long as the process of nutrition, or the formation of tissue, proceeds normally, the result is health, not disease.

The mode has much also to do with the quality of the resulting solid tissue. The phenomena of continued fevers are, probably, conservative in this, that they are intended to remove defective tissue; or at least, in grave cases, most of the solid tissues of the body at the commencement of the morbid phenomena are wasted before convalescence is established. Many causes, however, contribute to produce defective solid tissue, among which extreme variations of temperature in rapid succession are prominent. Velocity of formation has much to do, also, with the character of solid tissue. To decreased velocity of tissue-formation and loss of normal forms are due the phenomena and changes by which advancing life and old age are characterized.

As there is a time or moment at which formless fluid organic matter passes, through cells or granules, into solid organized tissue, so there is a time or a moment at which this solid tissue reassumes the fluid or gaseous form, or both, in its retrocession to lower states of organization. The agency by which this is certainly and mainly, though not, perhaps, wholly accomplished, is the oxygen introduced into the system at the lungs. Combining there with the organized hæmatin of the red disks of the blood, it is distributed to the regions of nutrition and oxidation, which are conceded to take place in the minutest capillaries of the blood-tubes. Of the results of these transformations, a portion is unfit for any other purpose in the system; the carbonic acid of the gaseous products finds exit at the lungs. Another portion, particularly of the alkalies, is arrested in the liver, where, after some not well-understood transformations, it is used to neutralize the acid of the chyme, as it arrives from the stomach in the duodenum; still another portion is arrested in the kidneys, further transformations take place, and, as organic salts dissolved in the water of the urine, pass out of the body through the bladder.

Over these processes, nutrition and waste, unquestionably therapeutic agents, do possess influence, modifying the mode and velocity of both processes.

These data would seem to establish that all therapeutic, hygienic, and remedial agencies, past, present, and future, can produce their effects only by promoting or retarding the generic totals of constructive and destructive metamorphosis, into which it is here proposed to arrange them.

As therapeutic agents apparently can only produce their effects by modifying the organic processes of repair and waste the causation of disease most likely acts in a similar manner; for, as has been stated, so long as these processes proceed normally, the result is health, not disease. In accordance with this conception, disease must consist in an altered condition of the normal velocity and relations to each other of the processes of tissue-making and tissue-disintegration. Referring to the diagram, it will be seen that it represents the disappearance of force so long as organization is ascending, or acquiring complexity; or, in other words, it requires force to effect organization, the organization itself being in this instance the correlative of force. As neither matter nor force is susceptible of annihilation, and as force disappears as organization acquires complexity, so force reappears as complex organization is retroceding to simple states. Every form of force manifested by or in the living organism must then be coincident with some change of matter. All the chemical, mechanical, sensational, emotional, thermal, and psychological phenomena of the living organism are coincident with the decay or destructive metamorphosis of some of its various tissues.

Contrary to the implied teachings of medical literature, no pathological condition can be higher than normal life. Inflammation is spoken of as "high," which makes a pathological condition, in conception, higher than normal life. No error has been more mischievous in its effects on medical practice, and this from personal experience in the past. Inflammation of every possible grade essentially consists of oxidation, or destructive metamorphosis of tissue at higher than normal velocity, and its tendency is to death, not to a condition in the part affected more elevated than normal life; for, if it were, it should be hailed, not as a calamity, demanding the interference of medical skill, with therapeutic agents, but as a blessing for which all should be devoutly thankful.

What are called, in the present classification of therapeutic agents, "stimulants," as alcohol, opium, etc., are in reality "retarders of destructive metamorphosis." Thus, the reality embodies a conception directly the reverse of that commonly entertained. Those known as "sedatives," singularly enough, act in the same way precisely as stimulants, viz., by "retarding destructive metamorphosis;" the difference between the conditions pointed out as appropriate for their administration, in works on therapeutics, being the presence or absence of mechanical results, and these depending on the condition of the nerve-masses. If the waste is confined to the general tissues, heat is the correlative, as indicated by the thermometer. If the nerve-masses are involved, there will be mechanical results in addition, as pain, spasms, convulsions, rapid and full circulation, etc.

The causation of disease must in some way primarily derange the normal relations of nutrition and oxidation as a totality, and before any local lesion can become apparent to our senses. There is not, and cannot be, such a thing as a local disease, with constructive and destructive metamorphosis proceeding elsewhere in the system normally. This is evidenced in many ways, as by change of temperature; and mechanical functions, as heart's action, loss of muscular power; loss of appetite, showing an arrest of nutrition; altered appearance of the surface, and expression of the face. Patients laboring under what is taught as a local disease, immediately lose the hue and color of health; the throes of the heart are more numerous than in health, or in some conditions less; the interior mucous surfaces undergo very great alterations, so far, at least, as vision can inspect them; the normal results of tissue-metamorphosis are no longer present in the excretions; the whole, not a part of the patient, is involved in the train of morbid phenomena.

With a sentient and ever-vigilant nervous system flashing impressions instantly all over the organism, such a thing as local disease, it would seem, is certainly impossible. A single aching tooth deranges the whole, not a part of the economy. A small abscess on the extremity of one of the fingers, or toes, involves the totals of nutrition and oxidation of the whole system. If disease or the effects of injuries were local, a crushed finger would not blanch the whole cutaneous surface, and incapacitate the sufferer from following usual occupations; nor would chloroform be needful to the surgeon in operations. Even moral impressions, very frequently, partially and temporarily arrest the whole train of life-phenomena, as in syncope, etc.

According to this physical basis of life, all therapeutic agents must necessarily be arranged as they are found by experience to influence the leading processes of repair and waste, which it is proposed to designate as follows:

Promoters and Retarders	
of Nutrition, or Constructive Metamorphosis.	of Oxidation, or Destructive Metamorphosis.

As each organized tissue has its own mode, time, and rate of nutrition and oxidation, these four leading classes may be subdivided just so many times as there are widely-different tissues to be influenced. For practical purposes this need not be very minute.

This classification satisfies all the requirements of exact science; and as, in chemistry, the nomenclature of compounds explains their chemical structure, so, in this classification of remedial agencies, the nomenclature itself conveys a definite conception of what each remedial agent does, and how it does it, in the living organism.[1]

As it lies outside the purposes of this memoir, on a single group of therapeutical agents, to arrange all remedial agencies in their appropriate places in the proposed classification, it is not here attempted, only so far as the special agents under consideration are concerned. They are "promoters of oxidation, or destructive metamorphosis." No difficulty will be encountered by any one familiar with existing knowledge in regard to what remedial agents actually accomplish, in the dynamics of organic life, in placing each therapeutic agent in its proper place. It does not suffice to be governed in deciding what are the effects of remedial agents, when stated by any of the existing names of classifications, as "stimulants," "alteratives," "emetics," "emmenagogues," etc., in works on general or special therapeutics, and current medical literature.

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[1] The physical units of physiological animal bodies are, for the material, form; and for force, motion. For, to evolve any of the phenomena of an organized being, food must be converted by the process of nutrition, into form; and motion must occur in the molecular structure of the form. Over forms, the records of operative surgery demonstrate that therapeutic remedial, or hygienic agencies have no other influence, save in exceptional instances, than destruction. But over the physical unit of force, motion, the power or influence of remedial agents is very great. This classification is, therefore, critically and scientifically correct, as it includes the whole of the physical unit of motion.

The inquiry in reference to any special agent must be strictly limited to what it actually does, in determining to which of the classes it properly belongs.

The following general principles are applicable as aids:

1. The reign of law is supreme in the living organism.

2. For every dynamic result special instrumentalities are found. Without nerve-masses, no intellectual, sensory, emotional, or mechanical results are possible; without muscles no mechanical results can take place; without nerve-cords no transmission of impressions from one part to another, etc.

3. Every dynamic result, mechanical, thermal, sensory, chemical, emotional, intellectual, and psychological, is coincident with and depends on change of matter, and bears the relations of cause and effect, and in every instance oxidation.

In other words, and more general: for every dynamic result there must be change of matter, whether in organic or inorganic natures; either as gravity, fall of water, chemical affinity, as combustion or oxidation—change of temperature, heat being correlated in currents of air, or the evaporation of water.

4. That the human organism is an integer, composed of many fractions, and nothing can affect one part without affecting the whole. It is far more truthfully and emphatically "*E pluribus unum*" than its counterpart in the political world.

5. That as the totals of nutrition and oxidation are interfered with by the causation of disease, as well as all therapeutic agencies, the position of any remedial agent in the proposed classification is to be determined by its more prominent effects on either nutrition or oxidation: thus, the influence of alcohol is to retard destructive metamorphosis in a much greater degree than constructive metamorphosis; therefore, its proper place is with the "retarders of oxidation, or destructive metamorphosis."

6. The mineral acids, and the salts they form with many of the metals, as iron, zinc, bismuth, copper, etc., play more or less conspicuous parts in constructive metamorphosis. In other words, the acid elements predominate in nutrition.

7. The fixed alkalies, and the salts they form with min-

eral and organic acids, after the oxygen taken into the system of the lungs, are potential agents in the process of oxidation or destructive metamorphosis.

Gravitation, the continuity of matter, and persistence of force, are general principles underlying physical science. There must be some hitherto unrecognized principle of like character underlying the causation of disease, and the operation of remedial agencies. It seems most probable that it concerns their general or constitutional action, in contradistinction to their at present recognized local character, in all cases whatsoever. Reasons for rejecting the separate entity of diseased action, and the adoption of the opposite conclusion of its unity or constitutional character in every instance, have already been given. Equally strong reasons exist for regarding the *modus operandi* of therapeutic agents in like manner.

Mankind have in all ages of the world witnessed the rapidity with which the organic poison of serpents spreads over the system, inserted by their fangs beneath the cuticle. The quantity which it is possible for a serpent thus to inject must be very small indeed, yet it is sufficient in a very brief time, in many instances, to prove fatal to life. Nor is the serpent alone in this mode of defence or revenge. Bees, wasps, hornets, and many other species of animals, are so provided. The injection of any of them is local for, at best, so brief a time, as hardly to be measured. It now seems strange that the idea of using remedial agents by inserting them beneath the cutis never occurred to any one before our own times. The use of certain remedial agents by hypodermic injection is now sufficiently common in most civilized countries as no longer to be regarded as a novelty, though its origin hardly dates back a decade of years. Certain agents introduced subcutaneously extend their influence over the system almost, if not quite, instantly. If disease and remedial agents were local in their operation, medicine would have been an exact science centuries since. The action of other agents by the lungs, as anæsthetics, by the mouth, as prussic acid, or cyanide of potassium, affects the general system with nearly equal promptness as by the hypodermic syringe. The recent proposal of Prof. Broadbent, of London, to treat cancerous and other morbid growths by the hypodermic injection of acetic acid, was unsuccessful only because the action of acetic acid could not be confined to the part injected. If there exists a single substance in Nature which does not affect the totals of nutrition and oxidation in the human body, it can serve no useful end as a therapeutic agent. For these, among many other reasons, it is impossible for me to regard either as local; and it further appears probable that these are the principles destined to educe order and certainty out of the existing uncertainty and confusion in practical therapeutics.

The relations between the living organism to be influenced, and therapeutic agents by which to influence it, are necessarily intimate and important. Without some definite understanding on these points, between author and reader, it would be impossible for the latter to judge conclusions properly. Each of the two factors has certain inherent properties, and relations to each other, retained through all mutations, and governed by fixed and unchangeable laws. Were it not so, the considerable space occupied in this memoir in considering the anatomy, physiology, pathology, and dynamics of the human body, would be out of place and unnecessary. It was seemingly demanded, because the physical basis of life, from which the physiological effects and therapeutical uses of the bromides are, in part, to be determined, is by no means generally recognized or accepted by the medical mind. The conclusions are, to a great extent, unsupported by "authority." The tendency of the medical mind is certainly in this direction, but not as yet recognized in schools or standard medical literature. But, that the physical body of man, over which alone therapeutic agents can exercise control, is composed of ordinary matter, whose elements are well known, and the equally well-known ordinary modes of the physical forces of the inorganic world, light, heat, electricity, magnetism, gravity, chemical affinity, etc., is certainly true. The sole fact unaccounted for is the forms of organic tissues and textures. The varied mechanical, thermal, chemical, sensory, emotional, intellectual, and psychological results are due to the varied organic instrumentalities, through and by which they are manifested.

The apparently summary dismissal of the so-called vital force from the domain of therapeutics is certainly warranted by the data here brought forward; and no fact or circumstance bearing on it has been purposely left out or avoided. The desire as well as design has been, from the beginning, to face squarely the whole problem, and to arrive at conclusions warranted only by all known facts, and indifferent as to what they should be, only that they should represent the truth.

Bromine is an elementary substance, sparingly diffused through Nature, having been found only in sea and mineral waters, and certain marine productions; and is a discovery of our own times (1826). In its pure state, it is highly caustic, and exceedingly obnoxious to all animal organic life. It is quite volatile, boiling at 117° F., and becoming solid at 4° below zero. It is analogous in many respects to iodine, with many points of resemblance to chlorine. It forms, like iodine and chlorine, acids with both hydrogen and oxygen, and combines directly with metals, and forms salts with the fixed alkalies and ammonia.

Bromides of potassium, sodium, ammonia, iron, and mercury, have been experimented with as therapeutic agents. But the preparation enjoying the most favor with physicians, and consequently most largely used, is the bromide of potassium. A concentrated solution of bromine is used as a caustic.

As the modes of preparing these different salts and bromine in solution, their sensible properties, etc., are accessible in elementary works, it is not deemed necessary here to reproduce them.

What are the physiological effects of the bromide of potassium? According to the data and classification of therapeutic agents here submitted, it is a "promoter of destructive metamorphosis." Its place is there theoretically, and the empirical results following its administration sustain the inference. It is composed of two elementary substances, bromine and potassium, both of which are, in their uncombined state, active potential caustics—that is, agents to break down organization.

It is not at all probable that it maintains its chemical character in the midst of the incessant molecular changes which constitute organic life. The elements cannot, by any new combinations they may form, change their inherent relations in that respect; and so must continue to increase the velocity of the retrograde metamorphosis of the tissues, and the oxidation and expulsion of effete matter. This view is fully sustained by careful analysis of the excretions during its administration. Though a very small portion finds exit at the kidneys unchanged, the greater part is excreted, wholly changed in chemical character. The part played by potassium in any of its manifold chemical combinations, in organic life, is very uniformly that of "promoting destructive metamorphosis," of muscular tissue more particularly. Soda, on the contrary, performs an important function of a somewhat different character, as, in combination with phosphoric acid—basic phosphoric acid—it combines with and conveys carbonic acid from the capillaries to the lungs, where it is liberated and finds exit from the body. While there is little or no increase of thirst for water, from even large doses of bromide of potassium, it is exactly the reverse with bromide of sodium—in that respect resembling chloride of sodium—and the amount of chlorides is remarkably increased by both in the urine, as well as the total quantity of urine, and most other results of tissue-metamorphosis except urea—the system tolerating the presence of the bromide of potassium, but expelling, with all possible haste, that of sodium. For some of the above facts credit is due to Surgeon J. H. Bill, U. S. A., who published a paper in *The American Journal of Medical Sciences* for July, 1868, containing the results of a series of experiments on his own person with bromide of potassium particularly, and incidentally with bromide of sodium, with a view to determining the anæsthetic and hypnotic properties alleged to be possessed by the former. The conclusions, mainly in his own language, are as follows: Variation of temperature, not worth recording; acidity of urine increased; chlorides excreted always notably increased; potassa excreted largely increased; bromides in urine hardly detectable; quantity of urine always increased, but without thirst; quantity of urea not affected; uric acid increased; carbonic acid decidedly increased; fæces diminished in weight. No totals are given, nor can any be deduced from his tables; but the enumeration of rational symptoms under large or toxic doses of bromide of potassium is as follows: Drowsiness; albumen; suffocative feeling; drowsiness, relieved by open air; albumen in marked quantity; great discomfort and restlessness; albumen present; fitful sleep; drowsiness and suffocative feeling; felt ill; great lassitude; albumen in large amount.

Surgeon Bill also instituted another series of experiments, with morphia and cannabis indica, showing results exactly opposite to those with the bromide of potassium—large decrease in the excreta, and the results of tissue-metamorphosis contained in them. The testimony of all his experiments is in favor of the classification of therapeutic agents proposed in this memoir.

Surgeon Bill's results confirm the safety of deducing the physiological effects from the inherent relations of the bromides to organic life; and empirical therapeutics still further confirm their correctness.

Physiologically and therapeutically, all the bromides (potassium, sodium, ammonia, iron, and mercury) are agents to promote waste—tissue-metamorphosis—and differ from each other only in the extent of their interference with nutrition, and their activity in promoting waste; the bromide of ammonia being the least active, and that of mercury most so. Their effects are more noticeable on abnormal tissue, or tissue of a type foreign to the human body, as enlarged glands, scrofula, cutaneous eruptions, syphilitic and others; and organic matter interstitially deposited in the tissues, below the normal dynamic grade of life. This latter condition is induced by the interference of some foreign substance with nutrition and waste, not immediately active, as in lead-colic and paralysis; or by deficient physical exercise in those of sedentary habits; or vicissitudes of temperature, as in intermittent fever; and many other causes.

An exaggerated illustration of the "nervous states," depending on the retention of effete matter in the system, is had in retention of urine. Even when it is only moderate, the typically "nervous condition" is most striking. An evacuation of the bladder by the catheter is followed by the most delightful tranquillity, drowsiness and sleep, with no secondary effects as drawbacks. To attribute to the catheter "anæsthetic properties," or "hypnotic effects," would only be true from a certain narrow point of view. Yet, on the surface, there are cause and effect very closely connected.

Another illustration of the "nervous condition" is seen in the so-called lead-poisoning, or "lead-colic." The general *malaise* and suffering are here known to depend on the presence of lead interstitially deposited in the tissues in an unknown chemical condition. Temporary relief may be and is obtained by opium, belladonna, chloroform, etc., which act by retarding destructive metamorphosis in the nerve-masses more particularly than elsewhere; permanent relief only when the lead, and the tissue it has rendered unfit for the purposes of life, are removed by destructive metamorphosis, under the existing nomenclature "absorption." If this could be accomplished as promptly as the removal of the retained contents of the bladder by the catheter, the same results would follow as speedily; delightful tranquillity, drowsiness, and sleep, would soon follow; but no agent has been found to do it, and in all probability never will.

By the aid of these extremes, the whole range of neuroses, not depending on structural changes, involving loss of form,[1] may, by analogy, be explained; and the explanations would most likely be correct, because empirical therapeutics prove them so.

An unknown something gives rise to a neuralgia—pain in a certain local part—the whole organism is involved, however, as evidenced by the pain, general nervousness, inability to sleep, etc. Opium, in a certain way, procures present relief. Unaided molecular activity may then bring permanent relief by removing the cause. But failing, and it often does, the bromides, by promoting molecular activity, remove the cause, and permanent relief is obtained in a different way. Such are, apparently, the facts in the so-called neuroses. But the bromides are by no means the only remedial agents for such conditions. The rough but sanitary effects of emeto-cathartics, as well as those of mercurials, salines, natural mineral-waters, violent exercise, etc., are all in more or less use, and with a large measure of success.

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[1] Lost form is one of the physical units of pathology.

Therapeutically, the bromides are never indicated except when there is something to be wasted. The abnormal tissue of an enlarged gland in scrofula is never changed to natural tissue. All that is abnormal must be wasted to give place to natural tissue, to be constructed in its stead. So of cutaneous tissue, with loss of type or form, constituting skin-diseases. No therapeutic agent can change it to its natural type. Some of them, as the bromides, may, and do, promote their waste. If normal tissue is constructed in its place, then there is recovery, but not otherwise. And the failure of the form-force to reproduce it in normal type is the reason why cutaneous diseases are an opprobrium of medical art and skill. No therapeutic measures that "cure" a case of chronic cutaneous disease, or enlarged glands, do so otherwise than by wasting the abnormal tissue. Syphilitic diseases are generally more successfully treated by agents that promote waste than those which promote repair; and these, separately, not unfrequently fail. But by combining "promoters of constructive and destructive metamorphosis," as in the bromide or iodide of iron, or quinia, better results frequently follow; always provided organic material is supplied with which to carry on the reconstructive processes. Not unfrequently, however, recovery can only be had by withholding entirely all wasting measures whatever, and in their stead using every possible means to "promote constructive metamorphosis," the inherent forces of the organism suffice for the substitution of normal for abnormal tissue, with complete recovery as the result. This is more likely to occur, however, when the full normal velocity of tissue-metamorphosis is maintained by physical exercise, or other favorable conditions, as by introducing extra quantities of oxygen into the system by what is called "superoxygenation," and perhaps other means, but none are so certain as hard labor in the open air and sunshine.

The following cases may serve to illustrate these general principles as applied in actual practice:

February, 1866, Mrs. ——, aged thirty-six; four children; weight 160 lbs.; well fed, clothed, and provided for, was seized with severe pain in the right groin, over the region of the ovary, tongue furred, appetite capricious, excited circulation, and general febrile condition. Had distinctly marked daily exacerbations of fever, which did not yield to quinia. Considering the case to be one of typho-malarial fever, she was put on salines, and a general sustaining course of management; having to resort frequently to anodynes by the mouth, and hypodermic syringe, to relieve the pain in the groin. This condition of things continued about three weeks, when partial convalescence was established; but with little or no relief from the pain in the groin. Patient would not permit blisters or cups to her groin, so had to use ointments and liniments. No satisfactory results were obtained, and a careful investigation of the case was again made, resulting in finding no lesion of the uterus or vagina; but the ovary on the painful side was certainly somewhat enlarged and tender. The temperature, circulation, and respiration, were nearly natural, and patient was in good flesh; and as on some days she escaped from pain altogether, the diagnosis fixed the origin of her suffering in the ovary. She now submitted to be leeched, and then cupped, and had one blister in the groin, with mercurial ointment and belladonna pretty constantly rubbed about the seat of pain. But still she did not get well, and had to use a good deal of opium to keep at all comfortable. At this stage of the case, from some reports in journals, about that time, of apparently similar cases being relieved by the bromide of potassium, she was put upon it, half a drachm daily, gradually increased to a drachm daily. It was commenced soon after a change, and continued until the next monthly period, she losing flesh all the time. This change was the most painful one she had ever had, and, as usual at such times, all treatment was discontinued but opium. When her change was over, it was evident she was very much worse, and a trip to the sea-shore was planned and carried into effect immediately. At the sea-shore she rapidly got worse, and at the end of a month returned home thoroughly disheartened, with the conviction deepening on her mind that she was going to die. Another investigation of her case, with counsel, was made, and previous diagnosis only modified by her wasted condition. Iron, zinc, bismuth, and good living, were prescribed, followed by rapid improvement in her general health. Her side and back were cupped occasionally, and she gradually became better in every respect, and has been able to attend to her ordinary duties, with only occasional returns of pain for a year past.

In this case, the rapid wasting of the patient under the use of the bromide of potassium was very striking. On her removal to the sea-shore, under saline influences still, the wasting made equally rapid progress. It appeared, at the time of her return, as if she would certainly die, unless the tide of her life was speedily turned.

Per contra. Having struggled through to light against all "authority" in therapeutics, the following case was treated:

Mrs. P., aged sixty, large, fleshy, well cared for, with a pulse too slow, though temperature and respiration nearly natural. Flesh a muddy, whitish, waxy appearance; has suffered with pains in the chest and stomach for some three years past. Is attacked more frequently in the night than in the day, but is liable to suffer at any time. Has thought from the beginning that there was something growing in her. Has had the services of many physicians, but received, from the opiates generally prescribed by them, only the most temporary relief. About a year since she went East for treatment, and during six months had the services of a popular physician in the city of Philadelphia, with about the same results as at home. Completely disheartened and discouraged, she returned, having made up her mind that she had a cancer, or some other mortal disease, and there was no relief for her. In this situation she came under my professional care. Diagnosticating in her case the retention of effete matter, interstitially deposited in her tissues, with a rate of tissue-metamorphosis altogether too slow, but without organic lesion, or loss of form, in any of her tissues, she was put on bromide of potassium, an ounce to a pint of water, tablespoonful in a tumbler of water three times a day. Simultaneously, she was to take citrate of iron in quinia, with saline laxatives. To have good, substantial diet, all her appetite would take. The hypodermic syringe was used a number of times, with solutions of morphia, and morphia and atropia to procure sleep.

She also had poultices of flaxseed-meal, with small portions of mustard to epigastrium and spine applied a good many nights. Subsequently quinia and belladonna, alternated with citrate of iron and quinia, were given, and a natural saline mineral-water substituted for the bromide. Persevering with the definite purpose in my mind of obtaining a higher velocity of tissue-change, and the wasting of present tissue, in the face of some discouragements, at the end of two months, she was fairly convalescent, and further treatment discontinued. A partial relapse took place in early winter, incident perhaps to her confinement indoors by unfavorable weather. Salines, with quinia, iron, and belladonna, soon reinstated her in her previous comfortable condition, which she continues to enjoy to the present time, having been seen within a week of this writing (February, 1870).

Miss B., June, 1868, very stout and fleshy, aged forty-four, takes but little exercise out of doors; suffers from shortness of breath, and her breath excessively fetid, and not owing to necrosed teeth; pains in her bowels and breast, with frequent vomiting soon after meals; is a very dainty eater, and is exceedingly "nervous." Rest at night very much broken; temperature andrespiration barely natural; pulse below the natural frequency. In her case the following diagnosis was made: All her organic processes are performed too slow; food does not go into tissue fast enough, and tissue waste much too slow; and her system, as a result, is loaded down with effete matter; but no organic changes, or loss of form in any of her tissues. She is literally "stuffed up."

Prescription: Bromide potassium, commence with half drachm, and gradually increase to a drachm daily, in large dilution, with occasional saline laxatives to carry off *débris* of tissue-metamorphosis. At the end of six weeks was very much better; color, appetite, and general appearance, better; has lost flesh, and sleeps much better. Is requested to walk more in the open air, and to discontinue medicine.

In concentrated solution, Dr. Goldsmith, U. S. Volunteers, first used bromine, with, perhaps, better results, in the gangrenous condition of wounds incident to hospitals, than any other single agent. In such conditions there are often deep burrowings along the fascia of muscles, and course of tendons, and in the cellular tissue leading from the external opening. Some of the results of what is called gangrenous action are, under such circumstances, entirely absorbed, probably in the form of cells of very low organization, which multiply in the system with great rapidity, appropriating to their use the very slender supply of nourishment which it is possible to introduce in such circumstances, completely arresting nutrition, and tending with great rapidity to death. In the application of the bromine, every sinus must be laid open, and all clearly dead tissue or tissue of lost form removed by knife or scissors, and the wound cleansed as far as possible. Charpie or oakum, or, in the absence of both, cotton, saturated with the concentrated solution of bromine, with the aid of wooden instruments is pressed into every recess, in such way as to come into contact with every part of the diseased surface. The wound is to be filled up level with surrounding parts, and a bandage applied over all. No metallic appliances, or instruments, can be used in its application, and great care should be taken that the fumes, so rapidly disengaged, should be carried away from patient and assistants, as they are extremely irritating to the lungs; watches, chains, rings, pins, studs, pocket-knives, and every metallic article, should be laid aside before using the bromine for these purposes. Owing to these disadvantages, it will not likely be used except in grave cases, and only then after other and less unpleasant measures have failed. Its value, under these circumstances, is due entirely to its capacity of destroying every vestige of tissue below the grade of normal life in the part, with perhaps some absorption and more or less rapid oxidation of the foreign matter in the blood.

From this physiological, pathological, and dynamic survey of the human organism, on a physical basis of life, and the inherent relations of certain classes of therapeutic agents to its organic processes and structures, and from the physiological effects and clinical uses of bromine and the bromides, the following general conclusions are deduced:

1. That, from the inherent relations of bromine, and the bromides, to the organic tissues and structures of the human body, their physiological and therapeutical effects must always

be that of promoting destructive metamorphosis, or waste: first, of all matter below the normal dynamic condition; second, of tissue or structure of type or form foreign to the human body; and, lastly, of the normal tissues themselves.

2. That they are never indicated, therapeutically, except when there is matter or tissue in the body, which it is desirable to eliminate from it.

3. That they possess neither inherited nor acquired anæsthetic properties, nor hypnotic effects, as chloroform, ether, opium, or cannabis indica, which act by retarding destructive metamorphosis; but may, by promoting destructive metamorphosis of retained effete matter, be followed indirectly by anæsthetic or hypnotic effects, in the same way as the evacuation of the bladder of retained urine by the catheter.

4. That they are contraindicated where nutrition is much impaired, or the rate of tissue-waste more than natural, and where structural changes, or loss of form, by substituting tissue of lower for that of higher organization, have impaired the dynamic integrity of the nerve-masses, as in locomotor ataxy, insanity, spinal paresis, etc.

5. That they are only indicated where the organic processes of life are restrained or interfered with, by adventitious circumstances, to resume their normal working on the removal of the restraint.

6. That their effects on persons living luxuriously, and leading inactive or sedentary lives, and on all in whom tissue-metamorphosis is sluggishly performed, is to increase the rate of waste, and to compensate, to some extent, for the physical exercise necessary to maintain the normal velocity of tissue-waste or changes.

Clinical Lectures on Mental Diseases.—In addition to the courses given in Great Britain, mentioned in our last number (p. 443), we notice that Dr. Sankey, the lecturer on mental diseases at University Medical College, London, is delivering, once a week, during the summer session, a clinical lecture at the Camberwell-House Asylum. These lectures are open to the members of the profession.

Proceedings of Societies.

MEDICAL SOCIETY OF THE COUNTY OF NEW YORK.

Stated Meeting, April 4, 1870.

Dr. George T. Elliot, President, in the chair.

The President announced the admission to membership of Drs. Henry E. Henderson, Horace T. Hanks, Joseph A. Monell, Simeon N. Leo, and Ralph L. Parsons. He reported the death, on the 2d instant, of Dr. Galen Carter, a member of the Society since 1828, and its President in 1853.

The report of the Committee on Intelligence was read by Dr. Thompson, and that of the Committee on Diseases by Dr. Raborg.

The weather being very inclement, and the attendance small, Dr. Austin Flint, Jr., moved that the reading of Dr. Lusk's paper on *Sugar-Formation in the Liver*, announced for this meeting, be postponed to a more favorable evening. Dr Lusk expressed his readiness to read the paper to the present audience, but Dr. Flint's motion prevailed, and the President then called upon the mover to occupy the hour, suggesting as his subject the

PHYSIOLOGICAL EFFECTS OF MUSCULAR EXERCISE.

Dr. Flint, Jr., said that, upon so sudden a call, he could not be expected to give the subject any elaborate or systematic treatment; but it was one to which he had devoted considerable attention, and he would present some points which, though not wholly new, might yet not be without interest.

It was hardly necessary to state that the nutrition of the body is carried on in obedience to the demand created by the waste of its different tissues; and the fact was familiar to all physiologists of the present day that a physiological increase of this waste, within certain limits, not only promotes a more active nutrition of the several tissues, but also serves to develop what may be called their vital properties. To consider only the muscular system, which constitutes so large a portion of the organism, we know that muscles called into frequent exercise increase in size and power—the finger-muscles of the pianist, for example, become extraordinarily developed—while muscles less exercised are correspondingly weak. If any muscle is entirely disused, it undergoes atrophy, and after a certain time becomes reduced to such a condition that it is incapable of regenerating itself, even though the blood may contain all the materials necessary for its full nutrition. Paralysis illustrates this. When the nervous supply of a muscle is cut off its fibres become weaker, its circulation is diminished, and after a time we find, under the microscope, that the proper muscular substance—the musculine—has been replaced by a material physiologically inert, fatty granulations. By-and-by these granulations so far take the place of the functioning muscular substance, that, even if the nerve-power be restored to it, the muscle cannot resume its function. The practitioner familiar with nervous diseases is well aware of this; and in a case of paralysis he first of all ascertains whether the affected muscles will respond to the stimulus of electricity. If they will so respond, then, even though all power of voluntary contraction be for the time lost, the methodical application of this stimulus will serve to maintain and increase nutrition, and, as a rule, the restoration of function is possible. But if it be impossible to make the muscles contract, however feebly, then there is little hope from any treatment. Diagnosticians go further, and, by the aid of a small trocar or harpoon, remove a little piece of the muscle for microscopic examination. In the muscles which show any contractility, they find the fibres with their characteristic striations, though mixed with the fatty granulations; while in those which show none, the fatty matter is found almost alone.

Muscular exercise increases the quantity of excrementitious matter produced in the system. On this point the speaker had no doubt, although there were some observations which might seem to show that exercise does not augment the amount of urea discharged in the twenty-four hours. Prominent among these were the well-known experiments of Fick and Wislicenus in their ascent of the Alps; and their results would seem lately to have been corroborated by Prof. Haughton. These observations, when first published, the speaker had been disposed to receive with a good deal of doubt, and to examine critically for sources of error. For if urea be the most important of the excrementitious principles, as regards quantity, and if it be not increased, but rather diminished, by violent exercise, then we must make over again our physiology. Fick and Wislicenus, instead of comparing the discharge of urea under ordinary conditions of exercise and diet with that after extraordinary exercise upon ordinary diet, compared it with that after extraordinary exercise upon non-nitrogenized diet. Yet it is well recognized that the amount of this excretion depends in great measure upon the character of the food ingested. Prof. Haughton's experiments were even more loosely conducted. He obtained his normal discharge of urea under ordinary conditions from observations made some years before, and compared this with the amount discharged (in the urine) under violent exercise taken in the heat of summer, and exciting profuse perspiration. Now, urea is a constant constituent of the sweat; and Speck and others have established the fact that, where violent exercise is taken under such conditions as to induce great diaphoresis, the amount of this principle eliminated by the kidneys may suffer no increase but even diminution, the skin relieving the kidneys of a portion of their work.

These experiments, therefore, by no means prove that muscular exercise does not increase the amount of urea discharged from the system. And in the same category are to be placed the observations of those who claim that extirpation of the kidneys does not produce accumulation of urea in the blood. If it is not established that urea is an excrementitious principle brought by the blood to the kidneys and simply eliminated by them—not *secreted* by the kidneys from materials furnished by the blood—then all our experimental physiology is of little worth.

It seems certain, then, that exercise increases the amount of effete matter eliminated. Within physiological limits, this increased elimination is attended, if proper nutriment be presented in the blood, by an increase in the activity of nutrition. If a man in perfect health, eating and drinking according to undepraved tastes, exercise his muscular system so as to increase to the highest physiological point the elimination of effete matter, he will correspondingly increase the nutrition of his muscular system. Nutrition consists in the appropriation by the tissues, from the blood, of new *functioning* matter—in the case of a muscle, of musculine, in place of the worn-out matter dicharged as urea, creatine, creatinine, etc.—not in the deposit of a functionless material, like fatty granulations. The more active this appropriation and the greater its amount, the higher will be the state of development of the muscle, and the better will it be able to do its work.

Now, if we can thus develop the functional capacity of the muscular system, and bring it to the highest point of efficiency, it is not unreasonable to suppose that the rest of the body will share in the improvement. We have, for example, an increased consumption of oxygen, an increased production of carbonic acid, and so increased functional capacity of the lungs.

Can a man, by such exercise, successfully combat the tendency to the deposit of foreign substances in the tissues; can he avert, for instance, the production of such heterologous formations as tubercle? The speaker, although from a limited experience, felt confident in answering "Yes." He had seen men of tuberculous diathesis, and with tubercle already deposited in their lungs, as testified by the best diagnosticians, go to the gymnasium and become hale and strong. He had in mind a young man, a patient of his father's, who had become a most accomplished gymnast after pulmonary tuberculosis had been clearly diagnosticated. In view of the principles before spoken of, it would seem that these men might, by increasing the deposition of normal material, have removed the disposition to the deposit of abnormal material. The speaker had, of late years, taken a special interest in examining this matter, for he had himself been subject to an affection which he thought might be relieved, and its effects averted, by systematic exercise. He had made trial of the plan, had been very constant and regular in his exercise, and had experienced the happiest results.

Let a man, moderately healthy, work as hard as he please with his brain—if only he take the appropriate exercise to keep the system throwing off all the effete matter, and appropriating, in place of it, healthy, nitrogenized, functioning matter, he will be comparatively safe. It is surely better to be thus recruiting every day, and keeping one's self in a state of constant efficiency, than to break down every few months or years, and have to take a vacation to build up again.

Dr. Neftel, being called upon, spoke of the high standing of Fick and Wislicenus among physicists and physiologists, and of the real object of their experiments, which was to prove that in muscular exercise hydrocarbons are chiefly consumed, and to compare the mechanical power so generated with the amount of heat produced by their oxidation. As to the reaction of paralyzed muscles to electricity, it depended much upon the cause of the paralysis. In cerebral paralysis, the electro-muscular contractility was increased, as shown by Marshall Hall. In spinal paralysis it was diminished, though to this there were exceptions. In peripheral paralysis it was diminished to the induced current, but often increased to the galvanic current. These differences were so characteristic as to serve for diagnosis of the place of lesion in paralysis.

Dr. Folsom inquired of Dr. Flint whether the increase of a muscle in size was due to a deposit of sarcous elements within the existing fibres, or to an addition of new fibres. The latter would seem to be a higher process, as implying an increase of the formative force of the body—that force which determined the shape and size of all its organs. He wished to ask also the effects of muscular exercise upon the vaso-motor nerves; and the comparative effects of light and heavy exercise.

Dr. Flint, Jr., said that the first question, as to the mechanism of a muscle's growth by exercise, he did not think it possible as yet to answer definitely; though the view most in favor with the best pathologists seemed to be that no new fibres are developed, but that the muscle increases in size by the increase in size of its existing fibres. It was certain that the muscular fibres of men were larger than those of women, and those of a strong man larger than those of a weak one.

The second question he would not attempt to answer. Our knowledge of the vaso-motor system was still too imperfect. As to the relative value of light and heavy gymnastics, there were great differences with different individuals. It was probable that the very light exercises, such as swinging wooden dumb-bells or the lightest clubs, would effect comparatively little in the way of muscular development. The very heavy exercises—lifting weights of six or eight hundred pounds between the legs, putting up dumb-bells of seventy, eighty, a hundred pounds—though developing enormous power in a special direction, did not well develop the system generally. The men who could do these feats could do these alone—they were stiffened for every thing else. The exercises which professional gymnasts found the best for their general development were those of a moderately heavy character continued for a long time—the pulling of weights, or the swinging of dumb-bells, weighing eight or ten pounds, for hundreds of times. Such exercises were very fatiguing, and their practice developed great endurance.

DR. CHADSEY had found well-directed exercise an invaluable therapeutic agent in numerous cases of pulmonary and other diseases, some of which he related. The exercise should be such as to increase the flow of blood to the diseased part, and in affections of the lungs such as to develop the muscles of respiration. In a case of advanced muscular atrophy, exercise, dry heat, and electricity, were bringing about a rapid restoration.

THE PRESIDENT was profoundly convinced that one of the most important elements of success in solving the difficult problem of the treatment of diseases of women, consisted in securing such a mode of life as would compel muscular activity and so equalize the circulation. For many patients he was in the habit of ordering localized movements, shampooing, etc., until they were able to take out-of-door exercise. In a large class of cases of anæmia and amenorrhœa, where the patients complained of weakness, wanting to keep in the house and loll on the sofa, he had more confidence in this physiological mode of treatment than in all others. If this were properly followed out, the local treatment now so much in vogue, and the ever-ready resort to the speculum, might commonly be dispensed with.

The President announced the names of the delegates to the American Medical Association, after which the meeting adjourned.

Adjourned Stated Meeting, April 18, 1870.

DR. GEORGE T. ELLIOT, President, in the chair.

INTRA-UTERINE MEDICATION.

DR. J. C. NOTT read a paper upon this subject, from notes of which we give a full abstract:

After a brief *résumé* of the leading points of an article which he had published in the *Journal of Obstetrics* (November, 1869), upon the various indications for intra-uterine treatment, the various means devised to meet them, and especially a new and safe instrument for uterine injection, the speaker said that his attention had been of late particularly drawn to the chemical effect, upon the bloody and other uterine discharges, of most of the agents commonly introduced into the uterus in the topical treatment of its diseases. The albumen, fibrin, and corpuscles of the blood, constituting, according to Flint, Jr., about four-fifths of its bulk (according to those who reject their water of constitution, about one-fifth), are coagulated, by some of the agents so used, into a solid mass. Most of these, indeed, have a greater or less coagulating effect, not only upon blood, but as well upon mucus and the various leucorrhœal discharges, which are highly albuminous. The important points are, that some kind of chemical reaction takes place between these agents and the fluids found in the uterus and vagina, and that this must greatly modify the effect of their application. A portion or the whole of the substance introduced will be neutralized and rendered inert by the discharges which it meets. Even chromic acid, for example, may be injected into a uterus filled with blood, and pour out of it without irritating the vagina, which is extremely sensitive to its action. Stillé mentions cases where pieces of lunar caustic have been swallowed without serious injury. Corrosive sublimate has such an affinity for albumen, that the white of egg is its best antidote. Tyler Smith says that leucorrhœal discharges, coagulated by injections, may remain in the vagina for days, and then come away in an egg-shaped mass. If this can take place in the vagina with its large opening, it is far more likely to occur in the body of the uterus, with its narrow outlet. That such coagulated masses were sometimes retained in the uterine cavity for an indefinite time, producing great irritation and protracted discharges, the speaker had had abundant occasion to observe.

If, then, inert chemical compounds are formed between the materials introduced, by injection or otherwise, into the uterus and the fluids found there, the extent to which the uterine mucous membrane is affected by the operation will depend upon the proportion between the two. And this will explain the discrepancies among gynæcologists regarding the value of various intra-uterine remedies. In this city the favorite method of application is by means of the probe, wrapped with cotton and dipped into a solution of the medicament. Dr. Sims uses a curved glass rod, without the cotton, and to this not more than two or three drops can adhere. The result of this treatment must be, in most cases, either negative or harmful. The greater part of the substance upon the probe will be rubbed off in passing the cervical canal. What remains will very likely be thrown into a puddle of albuminous fluid; or if the uterus be flexed, as is so often the case, it will probably come in contact with only one side of the organ, where the mucous membrane may be perfectly sound and so be only damaged by the application; or it may meet with a tuft of exposed vessels, and produce the most violent uterine colic, metritis, ovaritis, etc. It is essential to the proper effect of remedies that, preparatory to their introduction, the uterus be cleared out, as far as possible, by suction with the syringe. But often the surface is coated with a tenacious discharge, to remove which we must employ a weak solution of muriatic acid, common salt, or some other solvent of albuminous matter.

Led thus to the belief that intra-uterine medication had hitherto been most unsatisfactory, the speaker had made a series of experiments, which he hoped others might follow up, so as to place this practice upon a more firmly-established basis.

Styptics arrest hæmorrhage either by coagulating the blood or by constricting the vessels. To secure their action, it is necessary to keep them for some little time against the bleeding part; this an injection into the bleeding uterus often fails to do, though it may form a coagulum that will act as a tampon, and so stop the flow. The most effective way to control uterine hæmorrhage is to inject iodine, and then plug the cervix with cotton and persulphate of iron.

To determine the coagulating effect upon albumen of different reagents, the doctor had added to white of egg, in test-tubes, a few drops each of undiluted solution of persulphate of iron, saturated solution of chromic acid, same of tannin, same of nitrate of silver, pure carbolic acid, and Churchill's tincture of iodine. These experiments were repeated before the Society, and the test-tubes handed round. The iodine produced only a flocculent precipitate; but each of the other reagents combined at once with about its own bulk of albumen, in a firm, globular coagulum, which could neither be diffused throughout the mass nor increased in size by shaking.

Next, to imitate the most watery of the uterine leucorrhœal discharges, the white of egg was diluted with four parts of water, and submitted to the reagents as before.

Persulphate of iron (Squibb's solution) coagulated the whole mass into a jelly.

Saturated solution of chromic acid instantly formed a coagulum, less solid than before, but still too consistent to pass through a canula. Chromic acid the doctor considered a much more powerful intra-uterine remedy than persulphate of iron, and far more dangerous. If not neutralized by the secretions, it might produce the most terrible results. It was the most destructive to animal tissues of all agents used in medicine, and a strong solution would, in twenty minutes, completely dissolve a small animal. Even its most cautious introduction into the uterus, by the probe, was not free from risk.

Tannin produced a coagulum easily washed away, giving the mass only a syrupy consistence. This agent had, in common with persulphate of iron, the merit of being but slightly irritant.

Nitrate of silver produced a profuse, flocculent precipitate; and sulphate of copper had the same effect. It was generally agreed that nitrate of silver is one of the harshest applications to the uterus, particularly in the fluid form. The speaker had once injected a five-grain solution, and come so near killing his patient, that he had never experimented with it since. It was remarkable that it did not produce such severe results when applied to the uterine cavity in the solid form. To prove this, and to show how inefficient must be its ordinary superficial application, was read a long extract from Courty, who leaves the solid stick to dissolve in the uterus, as he claims, with much benefit and no danger.

Alum produced no coagulum, and so might probably be advantageously introduced into the uterus.

Saturated solution of carbolic acid in water produced a pulverulent precipitate. The doctor had found the injection of this solution give much pain, where that of iodine caused almost none. It was of much value, however, as an antiseptic.

The precipitate produced by iodine was not such as to interfere with the fluidity of the dilute albumen. There was no really potent remedy of which the uterus was so tolerant as of this. Even Churchill's tincture, undiluted, would be well borne where the most cautious use of nitrate of silver would give trouble. Of all intra-uterine remedies, iodine had most commanded the confidence of the profession. It was something more than a stimulant, a caustic, a styptic; it was a remedy *sui generis*, whose curative action could not be fully explained. The fact of its ready absorption made it especially valuable where we wished to affect the deeper tissues of the uterus, as in chronic inflammation. Nitrate of silver or chromic acid, on the other hand, had only a stimulant or caustic action; indeed, the absorption of the former would produce toxic effects.

In intra-uterine treatment we should begin by testing the tolerance of the organ, first with tepid water only, and then with a very weak solution of iodine. Though commonly almost destitute of sensibility, yet, under some conditions, the uterus becomes, like inflamed periosteum or peritonæum, exquisitely sensitive. But, even in these cases, it can usually be educated to bear injections of the requisite strength.

The result of a series of experiments upon blood was, in general, that most of the reagents above-mentioned produced a coagulum firmer than that with pure white of egg. As before stated, the coagulum formed in the uterus by persulphate of iron, though temporarily arresting hæmorrhage as a tampon, might afterward keep up the flow by its irritant action. In a case of metrorrhagia, close at hand, the speaker had used this styptic, by the advice of Dr. Sims. It had at once stopped the hæmorrhage, but for five days since a discharge had been taking place. Yet there were some cases where we must use the iron and hazard the risks.

With regard to the treatment of endometritis and its complications, gynæcologists might be divided into three schools—the cutting school, the cauterizing school, and those who use no intra-uterine treatment, but depend on vaginal injections, hygiene, and constitutional remedies. Perhaps a fourth school should be added—the eclectic, combining some elements of each of the others. Drs. Sims and Emmet were generally regarded as the representatives, *par excellence*, of the cutting school. The speaker had been much associated with them; and, though he had often heard them charged with needless use of the knife and scissors, yet he had rarely seen bad results from their operations. They were both extremely cautious in the intra-uterine use of caustics; and he did not hesitate to say, from actual observation, that caustics often do more harm than comes from cutting the cervix.

The doctor said that he had but half-finished his paper, and that the remaining part would be published.[1] He then exhibited his double canula for uterine injection. The injecting-tube was very small, and ran within the return-tube, which was four or five times its size, and had a very large eye, so placed as to be kept clear by the injected stream. Even with all this advantage, it was often impossible for the return-current to take place after the injection of some of the solutions above described, their coagulation of the uterine fluids was so complete and instantaneous. This was demonstrated upon a rubber uterus filled with albumen.

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[1] This "remaining part" was published in full in the June number of this JOURNAL.—EDITOR.

Dr. Fordyce Barker was anxious that a paper so original in its investigations and suggestions should receive a worthy discussion, and hoped that a meeting would be assigned for this after the completed article was in print. The subject was one of the greatest importance, and it was especially fitting that a full discussion of it should be brought out in this city, where intra-uterine medication might almost be said to have originated, and where it had been pushed to a more audacious extent than anywhere else. Previous to such discussion he would ask Dr. Nott to investigate the action of another agent, which had been his own special favorite for the last fifteen years—sulphate of zinc. He was wont to use it especially in those more or less trying cases of menorrhagia, so frequently associated with the climacteric period, and unconnected with prominent organic disease of the uterus, malignant or fibroid, although the organ was commonly enlarged. In some of these cases the flow would be profuse, in others long continued. For the treatment of excessive flooding in such cases he was accustomed to employ a paste made by rubbing up one ounce of sulphate of zinc with two drachms of glycerine. This was of just the right consistence to be easily introduced into the uterus through a canula the size of Simpson's sound. He thus introduced into the cavity from three to five grains of the paste; and in most cases one or two applications would be followed by entire cessation of the hæmorrhage. There was commonly no recurrence, and he had never seen any unpleasant symptoms result.

Dr. Peaslee could not agree to the analysis which gave eighty per cent. of the blood as organic matter, though this point was of little practical importance with reference to the subject in hand, since the very small percentage of fibrin was sufficient to coagulate the whole mass of blood for a time. As to the natural secretions of the uterus, cervix, and vagina, they were all mucous and contained not a trace of albumen; but the abnormal discharge from an inflamed mucous membrane was albuminous, and so the experiments presented bore directly upon the conditions met with in practice. The egg, in passing through the oviduct, obtained an albuminous envelope because the mucous membrane of the oviduct was in an unnatural condition of congestion. The speaker continued:

I am very glad to have a paper upon this subject from a man of the well-known independence and accuracy which characterize the writer of this one. And I cannot help thinking that such cautious remarks, from such a Nestor of the profession, must have their good effect upon some of its younger members, who, I fear, are led on too rapidly and recklessly by the brilliancy of intra-uterine medication. Just at this time the subject is become one of the most prominent, as it is one of the most important, before the profession; and, as Dr. Barker has remarked, New York is the field where its problems are to be worked out.

Dr. Kammerer had already shown that, if we would use intra-uterine medication with any certainty of success, we must first wash out the uterus by copious injections. Dr. Nott has to-night shown us the reason. Still the practice of introducing substances into the cavity of the uterus is, to a great extent, merely empirical. There are numerous points yet to be settled with regard to it. Many persons even now doubt whether such treatment should ever be resorted to; and though I think their doubts will be dispelled in the future, yet I will say that intra-uterine medication should be resorted to in comparatively very few of the cases of uterine disease, and, never except in those actually requiring it. What are those cases? What substances should be used? In what manner should they be used? Not one of these questions is yet settled. I have my own opinions, and should be willing to give the results of my experience, if the discussion is to take up the general subject of intra-uterine medication. But that might hardly be in order.

I have one or two points to suggest with reference to the nomenclature of uterine diseases. We need new terms with definite meanings. I would enlarge the anatomical nomenclature of the uterus, to correspond precisely with that we now use of the heart. I would speak of the *endometrium*, the *metrium*, and the *perimetrium*. Thence we have *endometritis*, *metritis*, and *perimetritis*. In intra-uterine medication we are seeking to make our applications to the endometrium, which can only be done, as Dr. Nott has said, by first getting it perfectly clean.

As an addendum to the doctor's remarks upon persulphate of iron (which is, I think, perfectly harmless—certainly so in metrorrhœa and metrorrhagia), I would say that in some cases it is only necessary to apply a plug to the cervix to get the tampon-effect which he describes. In cases of metrorrhagia attending the menopause, where I could not delay to send home for a syringe, I have taken a wad of cotton, tied a string to it, soaked it in a solution of persulphate of iron, letting it absorb all it would, and then passed it into the cervix. In the flaccid condition of the cervix at this period of life you can easily introduce such a plug, the size of a hickory-nut. It almost immediately causes contraction of the neck around it, and this contraction may continue for some hours, perhaps four. The tampon is perfect for that time, and the hæmorrhage, of course, is arrested; for the uterine walls are not distensible as in post-partum hæmorrhage, and you have put a stopper into the full bottle.

The injection of a solution of alum, a drachm to the ounce of water, I have once tried in a case similar to those first mentioned. It arrested the hæmorrhage instantly, and gave no after-trouble. I have also made use of Cagliari's (?) solution, of which alum is the chief ingredient, with similar result.

With regard to chromic acid, I had occasion to remark to the Obstetrical Society, three or four years since, that the only way in which I could explain the fact that cotton could be dipped into a saturated solution and passed into the uterus without harm, was that the acid never touched the endometrium at all. For if you pass a probe through the cervix, it is just as impossible for it to carry the acid to the endometrium, as for a big weasel to go through a small rat-hole without touching a hair. Still you do sometimes get ill effects from chromic acid. I have used it upon a few cases, and, though without ultimate bad results, yet with such symptoms as at first to give me much alarm. I do not know of any case where I should invoke its full effect, for I know of none where I should wish to remove the endometrium. I might be willing to touch a granulation with it, if I could be sure of touching that only; but of this one can never be sure. I find iodine perfectly safe and effective; so is the strong solution of persulphate of iron; so, too, are the saturated solutions of alum, and of tannic acid. I do not see, then, why I should make use of such an agent as chromic acid or nitrate of silver, except, perhaps, now and then in very weak solution. And I agree with the doctor, that we should in every case begin with a weak solution, until we see what the uterus will bear. I have seen chronic endometritis (or, as I should call it, chronic *congestion* of the endometrium) cured by weak solutions often repeated. In cases of retroflexion, I think the first thing in the treatment should be to replace the uterus; I should never think of first applying any thing within it, and should know of no surer way of producing adhesions of the organ in its false position than to apply chromic acid, which not rarely produces metritis. If there were adhesions already, and the uterus could not be replaced, then I would not use chromic acid, for other remedies would serve as well.

DR. EMMET.—I must confess myself very much astonished in listening to the remarks of Dr. Peaslee. I have probably employed chromic acid more than any one else, for the last fourteen or fifteen years, using it every day in the Woman's Hospital, and getting good effects from it. And now I feel very much as Rip Van Winkle did when he woke up, to be told that I have never been able to pass it into the uterus. Used with its own weight of water, I do not find it any stronger than nitrate of silver. I have never seen over half a dozen cases of bad effects following its use, and those were due rather to accident than to the remedy. I should prefer the nitrate of silver, were it not that the long-continued use of it hardens the tissues. Chromic acid, with an equal amount of water, is, I believe, no stronger; I have never seen it attack healthy tissue, or irritate the vagina any more than solution of iodine.

One remark further: I decidedly object to being considered a member of the "cutting school." I do not belong to any school. I suppose it would be impossible for two men to come to more opposite conclusions from the same data than Dr. Sims and myself. I claim to be eminently conservative; and, some time since,[1] I read before this Society a paper on "The Surgery of the Cervix," in which I endeavored to present these views. But, if you give a dog a bad name, it will stick to him. I then said that the largest number of times I had divided the cervix in any one year was twenty-three. Since reading that paper, I have operated five times. I never resort to surgical means except as a last resort, as it were; and I think many of those who do not make a specialty of uterine diseases operate a great deal more than I do.

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[1] February 8, 1869. The paper may be found in *American Journal of Obstetrics* for February, 1869, and report of the meeting in NEW YORK MEDICAL JOURNAL for April, 1869, vol. ix., p. 49.

Dr. Peaslee had no doubt that Dr. Emmet was so accustomed to the use of chromic acid that he was sure of getting it into the cavity.

Dr. Kammerer.—I presume, Mr. President, that Dr. Nott reckons me in his second class of practitioners—those who deal chiefly with uterine injections. If so, he is quite right, with the exception that of late I have a slight leaning toward the cutting side. For many years I have made this subject my special study, and I use uterine injections every day. By these I mean not only those into the cavity of the uterus, but also applications to the cervical canal. In fact, the cases which require local treatment of the uterine cavity are exceedingly rare, as compared with the numerous cases we observe daily where the cervical canal demands treatment. I am in the habit, nevertheless, of injecting tepid water into the cavity of the body, as a means of cleansing it, before making any application whatever; and this even where I only intend to treat the canal of the cervix, perhaps up to the internal orifice. After dilating the canal, I invariably make the injection of tepid water. I have been led to do this from knowledge and experience of the fact that albuminates do form whenever the secretions come in contact with certain salts and acids employed in the local treatment.

It is chiefly in cases of flexion, especially anteflexion, that the cavity of the uterus is often found dilated and filled with mucus. The contained fluid is rarely, however, of a purulent character; indeed, it rarely consists of thick mucus, but it is mostly clear, transparent, serum-like. This is in contradiction to what has been said by many others, but I nevertheless insist upon it. I am in the habit, as I said, of dilating the canal, and I frequently observe that, after cleansing the cervical cavity up to the internal orifice by a very small sponge or bits of cotton, then, on dilating the internal orifice, a teaspoonful or more of clear, serum-like fluid flows out, evidently from the cavity of the body. In these cases of catarrh of the uterine cavity, I have always insisted, and do now insist, that it is extremely important to keep the canal wide. We should learn from affections of the bladder how to treat an organ like the uterus. We observe that, when there is stricture of the urethra, the bladder becomes hypertrophied, and soon after the urine becomes stagnant. There is a similar condition of the uterus whenever there is stricture of the internal orifice; and you find in the cavity the thin mucus I have described. These cases often improve by simple dilatation of the internal orifice, repeated once or twice a week for a certain length of time. Gradually the hypersecretion diminishes, sometimes with no local treatment at all. But sometimes I have found that the contents of the cavity were not simply serous, but mucous and purulent; and in these cases I have invariably resorted to intra-uterine remedies.

My memory will not serve to bring up all the points that occurred to me during the reading of the paper; but a few facts I may state. That chromic acid and nitrate of silver coagulate not only the bloody but also the catarrhal secretions is a matter of common observation. Those who have treated hæmorrhages from erosions of the cervix, which are sometimes very considerable, know that, if they bring persulphate of iron or chromic acid in contact with the blood flowing from the erosion, large coagula of albuminates will form, which obstruct the vagina and may be difficult to remove. Of course if you introduce these agents into the cavity of the bleeding uterus, clots are formed which produce trouble before they are expelled. It is not necessarily the case, however, that a strong solution, injected into a cavity full of mucus, will simply form a neutral albuminate. If there be too little of the injection, the effect may be zero; but if there be more than enough to neutralize the secretions, then the caustic becomes active.

One part of Dr. Peaslee's remarks reminds me of a case that may serve to illustrate it. A lady, who had been bleeding for three months, came here for treatment, and was sent to me.

I examined her in the recumbent position. The uterus was somewhat enlarged, and the internal as well as the external orifice was moderately open. I introduced dilators, and, after full dilatation, injected cool water. To my great astonishment, a fearful hæmorrhage followed; while the patient was still there on the chair, the speculum filled with immense clots. I immediately resorted to a concentrated solution of chromic acid, which was injected into the cavity without the slightest effect; the hæmorrhage continued. I then made pressure upon the abdomen, and sponged out the vagina thoroughly, and thought I saw some erosions in the cervix. I took a brush and applied to them the chromic acid, and at once the hæmorrhage ceased. Now, if gentlemen think that by applying the plug to the cervical canal they have arrested hæmorrhage from the cavity of the uterus, I believe that they have mistaken cases of this kind for those of hæmorrhage from the cavity.

You know, Mr. President, that we have days when the devil tempts us to do just what we have often sworn we will never do. On such a day last winter, after reading what Courty and Tilt had recommended as the best treatment for uterine catarrh, I broke off a piece of nitrate of silver in the uterus, and resolved to make the trial. Now, when you break off, by accident, a piece of this caustic, and leave it in the canal of the cervix, you generally find little trouble. The fragment may be easily removed with the forceps. But, when you introduce the nitrate of silver into the uterine cavity, the result becomes very different, as this case and another will show. As I say, I introduced a piece of the nitrate into the cavity. The next day the patient sent for me, and I found her in a very bad way. She had been having chills, and presented symptoms of intense prostration. It was four or five days before I could consider her fairly out of danger.

The second case was rather worse. In this also I had introduced a piece of the solid lunar caustic into the cavity of the uterus. It was upon that same ill-fated day, last fall, when the Obstetrical Society was to meet at the house of Dr. Nott, that on going to the meeting I found a messenger from the patient awaiting me. She had been in excessive pain all day.

Within twenty-four hours she took two drachms of chloral hydrate, without any effect whatever. She then had two subcutaneous injections, which gave some relief. For several weeks she lay in a very critical condition. Periuterine inflammation was developed, with a hard mass in one broad ligament, and it took about three months to get over this. During the three or four weeks that she lay in bed, she had parotitis of the right side, and she has now paralysis of the facial nerve, which I hope is only temporary. I could attribute the parotitis to nothing but my daring application. The patient is now slowly recovering.

Dr. Whitehead had, in cases of slight endometritis, as detected by the probe, made frequent applications of a concentrated tincture of iodine. He applied it on the cotton-wrapped probe, and was sure it had reached the fundus. From this treatment he had gained not only amelioration but positive cure. He related a case that had been under his care at the Northwestern Dispensary for about three years. When first seen, the cervix was immensely enlarged; the uterus was in a state of fixed retroflexion, as carefully determined; and there was purulent endometritis. To the cervix he had made applications of chromic acid, and of nitrate of silver melted on the probe, with no bad effect except that the too frequent application of the nitrate produced constriction and finally obliteration of the os, requiring incision and dilatation. Since then he had applied strong solutions of iodine, and the patient had improved remarkably. He had, in one case, found such fearful prostration, with vomiting, etc., from the introduction of a probe tipped with the fused nitrate into the cavity of the uterus, that he had ever since been very cautious about using any thing but the strong iodine. He commonly employed the formula which was the favorite of Dr. Budd: ℞. Iodinii gr. lxxx., potassii iodidi ʒ ss., spts. rectificati f. ℥ i M.

Dr. Jacobi.—I am not expected to be an expert in these matters, and will occupy but a single minute. I wish to state that, so far as carbolic acid is concerned, I have used the impure acid, so called, undiluted, in many cases, and have never seen any bad effects from it. As to the nitrate of silver, the advantage it offers is that it will never act beyond the place where it is applied; and it is this advantage which has made it the pet of many practitioners. But I confess that, from this very fact, I have little faith in its producing any effect upon the whole lining membrane of the uterus, when introduced in the solid form. I think it can only affect the limited part with which it comes in contact. I would simply recall a case I had, years ago, where I dropped a piece of nitrate of silver into the right bronchus of a child, through a tracheotomy opening; and several days afterward had the chance to make a *post-mortem* examination. I found my piece of caustic upon the side of the bronchus, solid, embedded in albuminates, having done no harm. The very neighborhood of this piece of nitrate of silver was entirely intact. There was no inflammation, or even congestion, around it. I imagine, therefore, that in the uterus it would be the same—that its action would be strictly confined to the parts it touched.

Dr. Barker renewed his suggestion that a future meeting be assigned for the further discussion of the subject; and the President said that arrangements would be made to hear the conclusion of Dr. Nott's paper, and have it fully debated.

The Society then adjourned.

Stated Meeting, May 2, 1870.

Dr. George T. Elliot, President, in the chair.

The report of the Committee on Intelligence was read by Dr. Thompson, and that of the Committee on Diseases by Dr. Raborg.

ORIGIN OF DIABETES.

Dr. Wm. T. Lusk read the paper "On the Origin of Diabetes, with Some New Experiments regarding the Glycogenic Function of the Liver," which we present in another part of this number.

Dr. Austin Flint, Sen.—To make a beginning of the discussion, I offer a single observation. It sometimes happens that pathology communicates facts of importance in their bearing upon physiology, and it seems to me that in this instance we have an illustration of it. That in diabetes a change of diet, so as to eliminate in great measure the starchy and saccharine principles, has a very marked effect in diminishing the amount of sugar in the urine, is well known; though, perhaps, all may not have noticed the great extent to which the sugar is diminished. This fact seems to point to the view that the formation of sugar depends in no small degree upon the diet.

I have two cases of diabetes now under my care. In the first, which I have had under observation for a considerable time, the treatment has consisted alone in the regulation of the diet. On this treatment the urine has been reduced to its normal amount, and its specific gravity has fallen from 1040 to below 1030. Reagents still show the presence of sugar, but, were it not for this, the patient would consider himself perfectly well; indeed, he feels better than before he discovered his disease. The second case has been under observation but a short time, but even in that time the urine has been reduced in amount one-half, and in specific gravity from 1040 to 1030.

The dietetic treatment can be made very comfortable to the patient, if only the physician will take sufficient pains to explain the range of articles allowed, and the patient sufficient pains to prepare them. With proper variety and proper cooking, the diet is not unpleasant, notwithstanding the total exclusion of saccharine articles and the almost complete exclusion of amylaceous.

DR. GOUVERNEUR M. SMITH remarked that he had recently gathered some facts in relation to the pathology of diabetes, but was not as yet prepared to present them, since he was still investigating the subject, and anticipated making future use of the facts elicited.

In a communication received, in February last, from Dr. Dickinson, of London, the doctor had informed him that, from observations of five diabetic patients, it seemed to him there are constant and very peculiar changes in the cerebro-spinal system, resembling in some respects those found in the general paralysis of the insane; and this led him to the inference that diabetes is primarily a nervous disease. An abstract of Dr. Dickinson's views is to be found in the *Lancet* of February 19, 1870.

Dr. Smith would, on this occasion, only allude to two cases. One of them he had to-day seen—a man of active business habits, nervous, dyspeptic, addicted to excessive venery. He was so fond of sugar as constantly to carry *bonbons* about with him. It was a peculiarity in the case that the diuresis was not excessive. The patient had been for about two weeks on a treatment chiefly dietetic, and the glycosuria had diminished, as in the case just alluded to by Dr. Flint. The other case had been under his care within a year, and had terminated in a peculiar manner. The patient was a gentleman of means, who had been conspicuous for his mental activity and commercial enterprise. He had suffered with diabetes for several years. Leaving business, he travelled in Europe, and, while free from care, and using the alkaline waters of the German spas, he was relieved from the more distressing symptoms of his malady. He returned to this country, intending to make but a brief stay. His urine, examined about this time, still showed the presence of sugar. While preparing to retire, one evening, he pricked the bulb of the great toe with a tooth of a tortoise-shell pocket-comb which had broken off and fallen on the floor. This puncture excited a peculiar morbid condition, ending in mortification of the toe and finally of the entire foot. The gangrene partook of the senile character, and its progress was not rapid, the accident occurring April 5, 1869, and the patient dying from exhaustion on the 19th of the following July. Dr. Thomas M. Markoe early saw this case in consultation, and at a later period also Dr. Alfred C. Post. Gangrene of the lower extremities was occasionally a termination of diabetes, irrespective of any traumatic cause, Marsh, Colles, Marchal, and others reporting cases of this nature. No such termination, however, had ever been observed by the attendants in the case alluded to. As boils, carbuncles, and gangrene, were liable to occur during the course of diabetes, so, conversely, during the outbreak of boils and carbuncles there occasionally appeared a transient diabetes, such instances having been noted by Prout, Wagner, Vulpian, and others. This latter clinical fact was of interest in connection with the subject of glycogenesis.

Dr. J. C. Peters referred to a case of diabetes which had, in two attacks, been preceded by jaundice, indicating some connection of the disease with the liver. A case under his care last summer had surprisingly improved upon change of diet, and tincture of nux vomica, five drops thrice a day.

Dr. Peaslee.—This is a subject upon which, for twenty years past, I have thought more perhaps than upon any other in the whole domain of pathology; but I have come to no satisfactory conclusion. There are one or two points, however, which may aid us in coming to such a conclusion.

In the first place, while diabetes is so common in England and in this country, it is very uncommon in Germany. Some of our distinguished German *confrères* have told me that they had seen but two or three cases there in their lives. This would show that there are predisposing causes of great importance coming probably under the class of dietetic or hygienic influences.

I think we may conclude also that the cause of this disease is not always to be found in the same organ—that it must not be made to depend entirely upon a certain condition of the liver. Its pathology is probably not simple by any means. Doubtless there is more than one condition which may aid to produce it—more than one condition of the same organ, perhaps; or certain conditions which may affect different organs; or it may be the same condition of the different organs.

There is one point I think very probable, that the sugar is, at any rate, the result of disassimilation. The fact that, in the progress of the disease, there is such rapid emaciation, goes to show that the disease essentially consists in a development of sugar in consequence of disassimilation of tissue. Now, whether it be from disassimilation of the liver alone that we are having sugar formed all the while during our lives—whether from that alone, or from disassimilation of other parts also, remains to be seen. The sugar of milk is doubtless produced from disassimilation of the substance of the mammary gland itself, not by a definite act of secretion. And no more is the production of sugar in the liver the result of a special secretion of the organ; it is part of the normal process of nutrition.

It is a very interesting fact that the sugar of diabetes is grape-sugar, glucose, hepatic sugar—words all indicating a sugar identical in composition, though from different sources. Interesting, too, as showing that the affection has not one cause, but many, is the fact that the tincture of nux vomica, which we have all made use of in diabetes—for all of us who have been some years in practice have seen many cases of the disease—is in some cases valuable, in others not at all; and so with the tincture of chloride of iron, and with the alkalies. The fact that diabetes often follows icterus, as stated by Dr. Peters, and that it is necessary to modify the secretion of the liver—that is, to modify its nutrition—points in the same direction. The disease is due to several causes; and it will be for us to decide what is its pathology in each instance, and address our remedies to the organ affected. When we have left the narrow view that confines the origin of diabetes to one organ alone, we have taken the first step toward the true knowledge of its pathology.

Dr. Austin Flint, Jr.—I have naturally taken a great interest in this paper, not only from the importance of the physiological question involved, but also because it is a question in the investigation of which I have myself been very largely engaged. I would like to call the attention of the Society to one or two points in the paper, of very great importance, which probably the modesty of Dr. Lusk prevented him from putting forward as prominently as they deserve—for they are his own property.

The point of greatest interest is the process which he has fixed upon for the quantitative determination of sugar in the blood. The Fehling test is a very valuable one for the determination of sugar in the urine, where it is not masked by other principles, and where the use of the test is attended with no difficulty. But when this same test is applied to determining the quantity of sugar in the blood, it is impossible, by the ordinary method, to make an accurate quantitative estimate, for the reasons that the proportion of sugar is usually very minute, and that the process of extraction in a clear solution is somewhat complex. But, by adopting Dr. Lusk's method, of taking a number of test-tubes, each containing the same quantity of the test-liquid, and adding to each measured quantities of the clear extracts containing sugar, it is easy to see which of the test-liquids are the most completely decolorized, and present the greatest amount of precipitate, and in this way to arrive at a comparative estimate of the proportion of sugar. This method applied to two liquids cannot fail to give, in the most satisfactory manner, a definite idea of the relative proportion of sugar in each. I could only wish that Dr. Lusk had carried his process a little further, and separated and weighed the precipitate of copper. But perhaps this is not necessary in the present instance, in which the important point to determine is the preponderance of sugar in one or the other of the specimens examined.

I may, perhaps, be permitted to say a few words on my own experiments upon the glycogenic function of the liver. When I first began to teach physiology to medical classes, I found no difficulty in repeating the experiment of Bernard, and showing want of sugar in the portal blood, sugar in the substance of the liver, sugar in the hepatic blood, and in the right side of the heart. As, however, I gained in experience and experimental dexterity, I went through with the manipulations more rapidly; but the better I did it, the more unsatisfactory was the result, slight traces of sugar, only, appearing in the extract from the substance of the liver. The explanation occurred to me—and I believe the idea is original with myself—that, during life, the liver is constantly producing sugar, but, that the blood is as constantly washing it out. This explanation was suggested by the experiment of washing the liver free from sugar by a current of water passed through the vessels, letting it stand awhile, and then finding sugar which had formed again in its substance.

Surveying the field to get the point of departure for further research, the view of Pavy and that of Bernard seemed to be the only two entitled to consideration. Bernard seemed to lack strength in the fact that, while Pavy said positively that there was no sugar in the liver during life, he never took the liver under the conditions prescribed by Pavy, and established the fact of the presence or absence of sugar; and Pavy was weak in this, that while Bernard said the blood of the hepatic vein invariably contained sugar, and the portal blood none, Pavy never applied the accurate quantitative test to the blood taken from these different vessels. Had he applied the test used by Dr. Lusk, he could not have failed to admit the preponderance of sugar in the blood of the hepatic veins. Pavy seemed unphilosophical, also, in assuming, without reserve, that the sugar in the blood was inevitably the consequence of *post-mortem* change, and allowing this idea to influence all his deductions. In some of his experiments he adopted the very rough process of injecting a solution of potash into the portal blood, which he said would arrest the production of sugar—an experiment in which the conditions were manifestly unphysiological.

It seemed to me a simple enough question to find out the truth of the matter. If the liver be constantly producing sugar during life, we ought certainly to be able to detect it. I set about my experiments in what seemed to me to be a simple, common-sense way. I had a dog for a long while in my possession, so that he was accustomed to me, and could be handled freely. This animal was fed exclusively upon meat. In making the experiment, I placed him gently upon the operating-table, keeping him perfectly tranquil; had him held on his back by assistants, and, without the use of an anæsthetic, while the animal was perfectly quiet, I opened the abdomen in the whole length of the linea alba by a single sweep of the knife. I then cut off a piece of the liver, rinsed it from blood in a vessel of cold water, and cut it up into boiling water. The time between the first incision and cutting up the liver into boiling water was ten seconds. To all intents and purposes this was a living liver, and it is well known that boiling water prevents any *post-mortem* change of glycogenic matter into sugar. I never found a particle of sugar in an extract of the liver thus treated; and so Pavy seemed certainly right on this point.

The next question was to confirm the observations of Bernard upon the blood. I made several experiments upon this point in the following manner: I had the animal, a dog, held on his back and kept perfectly tranquil. I first broke up the medulla oblongata; then applied a ligature to the portal vein as it enters the liver; then opened the abdomen and chest freely, cut out a portion of the liver, and cut it up into boiling water; I then applied a ligature above the renal veins in the abdomen, and a ligature above the hepatic veins in the chest, and collected the blood from the portal veins and the blood coming from the liver. The entire operation in one experiment, which I have published in my work on physiology, only occupied sixty seconds. I found no sugar in the portal vein, and I found sugar in the hepatic vein. The experiment I allude to was performed before the class of the Bellevue Hospital Medical College, and in the presence of Dr. Lusk.

This experiment, which I have verified time and again, entirely confirms the experiments of Bernard and others, and I consider the question settled, that sugar is not contained in the blood of the portal vein in animals confined to nitrogenized food, but is always contained in the blood of the hepatic veins. It seems to me that I have settled the question, also, that sugar is not contained in the substance of the liver during life; but that it is none the less formed in the liver during life, being washed out by the current of blood as it passes through, appearing, of course, in the blood of the hepatic veins. These facts harmonize the apparently discordant observations of Bernard and of Pavy, and give the first accurate notion of the exact mechanism of glycogenesis which is in accordance with all the well-established experimental facts.

With regard to the position taken by Dr. Peaslee, I can see no reason for regarding sugar as a product of disassimilation, and I can see every reason for considering it an alimentary substance. In my experiments upon cholesterine, I assumed that it was necessary to ascertain that this principle is actually discharged from the body, in order to prove it to be an excrementitious substance, or a product of disassimilation. If sugar be a product of disassimilation, it is a waste product. All such products are gotten rid of, cast off from the body. Sugar is never discharged from the body in health; sugar is taken as food; sugar is formed in the body. No one would call fat a product of disassimilation; and fat is taken as food; fat is formed in the body, as is sugar, and, in its relations to nutrition, presents many analogies with sugar. Fat is never discharged from the body in health.

There is an exception to this rule as regards the discharge of sugar and fat, and that is in the milk; and here we have a most curious departure from the general laws affecting the mechanism of secretion. It is a recognized fact that glandular epithelium is necessary for secretion; the remarkable exception is in the mammary glands. These glands, rudimentary at birth, and in the male rudimentary throughout life, undergo a change in their development at puberty, but then are incapable of producing milk; they undergo another change during uterine gestation, and it is only in the latter part of this that they can secrete milk. When the secretion of milk is fully established, there are no secreting cells lining the glandular acini; and the bare membrane seems to be the agent which forms the butter, casein, and sugar. This, far from showing sugar to be an excrementitious substance, shows it to be a principle very necessary for the nourishment of the infant.

The observations which I made in 1868–'69;[1] their repetition soon after by Dieffenbach, who did not know of my own experiments; and the additional confirmation they have just received at the hands of Dr. Lusk, I think, settle the question concerning the sugar-forming function of the liver.

Dr. Peaslee.—If Dr. Flint considers cholesterine a product of disassimilation, because it leaves the body after its conversion into another product, might we not also regard sugar as such a product, if we can find substances leaving the body into which the sugar is convertible?

Dr. Flint, Jr., replied to the question about cholesterine, by giving his argument to *prove* the change of cholesterine into stercorine. Dr. Peaslee would be perfectly right if he could prove that the sugar was converted into any other substance which was discharged from the body. But Dr. Flint knew of no such substance into which sugar was convertible except carbonic acid, which, it was true, was diminished in diabetes. He also agreed with Dr. Peaslee, that we do not know what sugar does in the economy. We do not know that it forms any tissue. He regarded it as entirely analogous to fat, which is introduced into the body, consumed in the body, never discharged in health. They performed some function in nutrition, but what, he had not the slightest idea, and he thought the physiologists of the present day had not.

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[1] See this Journal, January, 1869, vol. viii., p. 373.

Dr. Chadsey asked what became of the sugar when taken as food in excessive amount. He had a patient who used to take some five or six pounds of sugar a day—thought he could not live without it. But no sugar could be detected in the urine; there was no diabetes. Some two or three years after the doctor knew him, this man met with an injury to his leg. The limb mortified, and he died. Dr. C. had attributed the mortification to the great amount of sugar he ate. Lately he had had a child as a patient, that he found breaking out with little sores, which mortified; and he discovered that the child was always eating candy.

Dr. Flint, Jr.—If I do not know what becomes of the sugar taken normally as food, I am very sure I do not know what becomes of the excess in the abnormal condition where it is craved. I judge that, in such cases, it is often disposed of in the same way that the ordinary amount is disposed of. It is certain, however, that diabetes has been produced in animals by feeding them with sugar.

THE HARVEY STATUE.

Miscellaneous business being in order, the Special Committee, appointed at the meeting of 21st of February, to inquire into the expediency of recommending that a bronze copy of Horatio Stone's statue of Harvey be placed in Central Park, presented a report in favor of the proposition, and the Committee was, by vote of the Society, empowered to appeal to the public for funds to consummate it.

The Society adjourned.

Bibliographical and Literary Notes.

The title-page of this well-known work[1] has, since the death of its author, become rather cumbrous by the successive editorial accretions which have marked the several editions, and increased their value. In the present edition, the several articles on general anatomy, previously scattered through the volume, and preliminary to each system, are gathered into an introductory chapter, and in part rewritten, by Mr. Holmes; and they give to the general student a very fair idea of the fancies and facts generally accepted under the title of histology. An excuse, or rather an explanation, is given for the introduction of a lot of old illustrations, which, whether correct or not, do not illustrate present teachings of minute structure, and had much better have been left out. And here we should state that nearly all the woodcuts in this American reprint are well worn, and should not have been used. Some of them are so indistinct as to be worthless.

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[1] Anatomy, Descriptive and Surgical. By Henry Gray, F. R. S. The Drawings by H. V. Carter, M. D. With additional Drawings in the second

Of the work itself it is hardly necessary to say a word. As the only one of its qualities in the English language—we speak particularly of fulness, and exactness, and clearness—it is a necessity; and this circumstance, with its immense sales, past, present, and to come, might have excused a little less economy in outfit on the part of the publishers.

In these days of superficial study it is really refreshing to come across a work[1] that evinces so much honest labor and painstaking research as the report before us. The whole field of pharmacy has been thoroughly worked over, and the result is an abstract of every thing of importance that has transpired within the year in the science of pharmacy. Much of the material collated has a value for the physician, and we have in another place made a number of brief extracts from the report. Our own profession might take an instructive lesson from the solid manner in which this report is put together,

and later editions, by Dr. Westmacott. With an Introduction on General Anatomy and Development, by T. Holmes, M. A. Cantab. A new American, from the fifth and enlarged English edition. With Four Hundred and Sixty-two Engravings on Wood. Philadelphia: Henry C. Lea. 1870, 8vo, pp. 876.

[1] Annual Report on the Progress of Pharmacy. Read before the American Pharmaceutical Association, at its seventeenth annual meeting, at Chicago, September, 1869. By Frederick Hoffmann, Ph. D. Pamphlet, pp. 163. (From the Author.)

and thus reflect greater credit upon themselves than can now be derived from many of their windy essays, whose value, if they have any, is wasted or swallowed up in their immensity of words.

A CAREFUL and prolonged examination of this volume of reports[1] from St. Thomas's Hospital has convinced us that it possesses features which, for practical purposes, give it many advantages over the ordinary so-called hospital reports. These reports, as our readers are aware, generally consist of elaborate and many times exhaustive essays on special subjects, suggested, as a rule, by one or more cases that may have come under the writer's observation. The report under consideration is quite different from this. It gives in a few lines the history, treatment, and termination of *every* case that has been in the hospital during the period indicated. Part I. consists of statistical tables, summing up and classifying cases by numbers only; while Part II. gives, under each disease or injury, the brief records above noted. As a sample of the manner in which the work is done, we select from the first disease on the list—chorea—five cases out of forty that are mentioned.

1. M.; æt. 9; ill 1 mo.; 1st attack; in hosp. 53 d. —Liq. Pot. Ars. ♏v, Dec. Cinch. ℥j. t. d.;—cold shower-bath. Cured.

10. F.; æt. 16; ill 18 d.; 1st attack, had rheumatism about her for 6 mos., heart-sounds healthy, right side chiefly affected; in hosp. 31 d.;—Liq. Pot. Ars. ♏iij, Inf. Casc. ℥j, t. d., 11 d.; Pot. Bicarb. gr. x, Pot. Nit. gr. v, Inf. Calumb. ʒj, t. d., 19 d.; Pil. Dov. c. Hyosc. gr. v, o. n., 17 d. Cured.

16 F.; æt. 57; ill 4 mos.; 1st attack, commenced after a severe fright —head, neck, face, and upper extremities affected—heart-sounds flat, but without murmur—she improved in general health and strength during her stay, but the choreic symptoms showed little improvement—she became a little steadier under the arsenic, but it occasioned severe headache; in hosp. 5 mos.;—Ferri Carb. c. Sacch. from gr. x to gr. xxv, t. d., 17 d.; Liq. Pot. Ars. from ♏iv. to ♏viij, t. d., 38 d.; Zinc. Sulph. ex Inf. Val. from gr. xj to gr. xx, t. d., 3 mos. Relieved.

27. F.; æt. 13; ill 4 mos.; 1st attack, mitral murmur, fatuous expression—a week after admission had 3 epileptic fits, and was removed shortly afterward by parents; in hosp. 10 d.;—Ferri Carb. c. Sacch. gr. x, t. d.;—shower-bath daily. Relieved.

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[1] Report of Patients treated in St. Thomas's Hospital from 1861–1865. London: John Churchill & Sons. 1869. 8vo, pp. 156.

39. F.; æt. 15; ill 1 mo.: 1st attack, attributed to fright—choreic movements general, heart-sounds healthy, pulse at first not frequent but feeble, tongue at first clean and moist, but latterly dry and brown, and symptoms of asthenia became general, the convulsive movements slightly affecting neck and face—bed-sores occurred from the friction; in hosp. 34 d.;—Zinc. Sulph. gr. ij, 3 d.; Zinc. Sulph. gr. iv, Ext. Belladon. gr. ¼, 7 d.; Liq. Pot. Ars. ♏iv, Dec. Cinch. ℥j, 11 d.; Dec. Cinch. c. Tr. Hyosc. ♏xv, 8 d.; Dec. Cinch. c. Ext. Conii gr. v, 4 d.; Dec. Cinch. c. Liq. Morph. Mur. ♏v, 3 d.; Tr. Cinch. ʒj, Liq. Morph. Mur. ♏iij, Dec. Cinch. ℥j, 4tis, 4 d.; Haust. Morph. Mur. (♏xx.), si opus sit; wine increased from ℥ij to ℥iij. Died.

Then follows a summary of the whole list, which we give entire:

Of the 40 cases treated, 13 were males, 27 females.

Of the 13 males, 12 were cured and 1 relieved.

Of the 27 females, 22 were cured, 3 relieved, and 2 died.

Of the cures,	10	were treated by	arsenic	alone ;	av'ge stay	in hosp.	29.4	days.
"	5	"	zinc		"	"	35.6	"
"	9	"	iron	"	"	"	40	"
"	2	"	zinc and iron		"	"	56	"
"	3	"	iron and arsenic			"	48.6	"
"	2	"	zinc, iron, and arsenic			"	99.5	"

One case was admitted at the age of 5; 2 at 6; 4 at 7; 4 at 8; 2 at 9; 8 at 10; 4 at 11; 6 at 12; 2 at 13; 2 at 15; 1 at 16; 1 at 17; 2 at 18; and 1 at 57.

This plan is followed throughout the entire volume, similar condensed statements being given for every case, whether of disease, injury, or surgical operation.

Doubtless the immense mass of material in our own large hospitals could be thus condensed, and rendered of some practical value to the profession at large. At all events, it would afford most interesting study for all who place any confidence in the numerical method of reasoning.

We are not certain that there is any pressing need of a treatise devoted specially to diseases of the heart[1] as occurring in infancy. We have, however, read Dr. Blache's *brochure* with much satisfaction. He treats of pericarditis and endocarditis, valvular lesions and enlargement of the heart, concisely and clearly; but, in the main, the treatise is applicable to adult age as well as to infantile life.

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[1] Essai sur les Maladies du Cœur chez les enfants, par le Dr. H. René Blache, Ancien interne en Médecine et en Chirurgie des hôpitaux de Paris, etc. 1869. Paris. 8vo, pp. 224.

The author appears to consider that affections of the heart existing in infants arise from causes acting after birth oftener than is generally supposed, although he recognizes the fact of their dependence, in certain cases, on disease in fœtal life. He also recognizes the well-known fact that valvular lesions originating before birth are generally seated in the right side of the heart; but he appears not to be acquainted with the *rationale* of this as set forth by B. W. Richardson. Dr. Richardson's very interesting and valuable experiments, showing the production of endocarditis by injecting lactic acid into the peritoneal cavity, are not referred to. This is the more noteworthy, as the copious bibliographical index appended to the treatise contains a fair proportion of English contributions. We cannot but remark that this index contains but a single American reference, namely, to an article on aneurism in the *American Journal of Medical Sciences.* It is fair to remark this, in view of the considerable number of English, German, and French contributions which constitute the list referred to.

Notwithstanding that American medical literature is almost wholly ignored, we are bound to say that the work is a very creditable production.

MESSRS. J. B. LIPPINCOTT & Co., Philadelphia, announce the first volume of the revised edition of Reynolds's System of Medicine.

FROM the press of Hurd & Houghton has appeared another volume of the Sanitary Commission Series. This volume is devoted to surgery, and is edited by Prof. F. H. Hamilton, M. D., of this city.

MR. HENRY C. LEA, of Philadelphia, has nearly ready "Chemistry; General, Medical, and Pharmaceutical," including the Chemistry of the British Pharmacopœia, with additions adapting the work to the United States Pharmacopœia, by John Attfield.

VOLUME III. of Trousseau's Lectures on Clinical Medicine is announced by Messrs. Lindsay & Blakiston.

BOOKS AND PAMPHLETS RECEIVED.—Notes on the Physiology and Pathology of the Nervous System, with Reference to Clinical Medicine. *A.* Disseminated Sclerosis of the Brain and Spinal Cord. *B.* Annular or Cortical Sclerosis of the Spinal Cord. By Meredith Clymer, M. D. Univ. Penn., etc. Pamphlet, p. 53. (From the Author.)

Report of the Committee on Medical Education. Reprinted from the Transactions of the American Medical Association, 1869. Pamphlet, pp. 30.

Report on the Epidemics of California in 1868. By F. W. Hatch, M. D. Reprinted from the Transactions of the American Medical Association, 1869. Pamphlet, pp. 34.

Miscellaneous and Scientific Notes.

DEATH OF DR. AUSIAS-TURENNE.—This physician, so well known by his experiments and writings on syphilization, died in Paris, on the 28th of May, aged fifty-nine. He has bequeathed his bones to Dr. Boeck, of Christiania, Norway, who is now visiting this country.

TO MASK THE NAUSEOUS TASTE OF SULPHATE OF MAGNESIA.—Mr. Isaac W. Smith, pharmaceutist, of Philadelphia, says the best plan is, to—

Take of liquorice-root, contused (deprived of outer bark), 4 troy ounces; boiling water, 2 pints, or sufficient. Mix and allow to stand, with occasional stirring, until cold; then press through muslin, adding more water, if necessary, until the residue no longer tastes, then filter; to the filtrate add Epsom salt, 4 troy ounces, and evaporate to dryness over a water bath. Each ounce of the compound represents about one ounce of the crystallized salt..—*Proceedings American Pharmaceutical Association*, 1869.

THE Mortality of Limb Amputations as regulated by the Size of Hospitals and the Degree in which Patients are aggregated or isolated. By Sir James Y. Simpson. [Edinburgh Medical Journal, December, 1869.]

The following extract is but a brief paragraph from Sir James Simpson's lengthy and valuable paper on the subject of "Hospitalism and its Effects." The data seem almost incontestable; but we have to add that the views of this distinguished authority on the general question of the good or evil of large hospitals have met with a very decided opposition in some eminent quarters, and a lively discussion has been going on for some months on this subject in several of the English medical periodicals:

> In the two previous parts of these communications on hospitalism we have seen that in our large metropolitan hospitals about forty-one in every hundred operated on die of those patients who are subjected to the four major [thigh, leg, arm, and forearm.—E. S. D.] amputations of the limbs; while in single or isolated rooms in country practice patients die, under the very same class of operations, to the extent only of ten or eleven in every hundred. In the last chapter we have collected evidence of the death-rate from these same four major limb-amputations in British provincial hospitals of various sizes. If we throw the whole facts thus collected into a tabulated form, the general results may be stated as follows:

Size of Hospital.	*Death Rate.*
1*st Series.*—In large and metropolitan British hospitals, chiefly containing from 300 to 500 beds or upward, out of 2,089 limb-amputations 855 died, or	1 in 2.4
2*d Series.*—In provincial hospitals containing from 201 to 300 beds, out of 803 limb-amputations 228 died, or . . .	1 in 3.5
3*d Series.*—In provincial hospitals containing from 101 to 200 beds, out of 1,370 limb-amputations 301 died, or . .	1 in 4.4
4*th Series.*—In provincial hospitals containing from 26 to 100 beds, out of 761 limb-amputations 134 died, or . .	1 in 5.6
5*th Series.*—In provincial hospitals containing 25 beds or under, out of 143 limb-amputations 20 died, or	1 in 7.1
6*th Series.*—In British private country practice, with the patients operated on in single or isolated rooms, out of 2,098 limb-amputations 226 died, or	1 in 9.2

> These data go to point out and establish the general fact or general law in hospital hygiene, that the death-rate accompanying amputation of the limbs—and, as we may infer, the death-rate accompanying other surgical operations, and many medical diseases also—is regulated in a great and marked manner by the size of the hospitals, and the degree of aggregation or segregation in which the patients are treated. But, like all other general laws in medicine, this law is subject to many exceptions. Thus, a small hospital, if overcrowded with beds and patients, becomes as insalubrious as a large hospital under one roof. On the other hand, a large hospital would be generally made almost as salubrious as a small institution, provided few beds were left scattered over its wards, and these wards were well ventilated and often changed. But such exceptions only establish more surely the great and important hygienic law, that, in the treatment of the sick, there is ever danger in their aggregation, and safety only in their segregation; and that our hospitals should be constructed so as to avoid as far as possible the former, and secure as far as possible the latter condition.

"The Sayre Malpractice Case."—We take from the law columns of the *World*, of June 19, 1870, the following report of this case, which has attracted so much attention among the profession of this city. Such a righteous verdict as this will go far toward doing away with the tendency which is so common among certain lay-people to attempt to extort money from physicians and surgeons for alleged malpractice. The allowance given to the defendant is the extremest amount authorized by law, though it is by no means probable that it will reimburse Dr. Sayre for his expenditure of money alone in defending the suit, to say nothing of the demands made upon his time in collecting and arranging evidence, etc. We have but a single regret to offer in view of the happy termination of this suit, and that is, that those who instigate such proceedings could not be made to suffer an equally severe penalty.

SUPERIOR COURT—SPECIAL TERM.—Before Judge Freedman.

Margaret S. Welch, by her Guardian, John F. Welch, *vs.* Lewis A. Sayre. —This action, it will be remembered, was brought to recover twenty thousand dollars damages for alleged malpractice upon the person of the plaintiff, a child then (April, 1868), about six years old, who was suffering from an abscess in the neighborhood of her left hip. Application was made to Dr. Sayre, the defendant, a well-known physician, of long standing in this city, who performed the operation, puncturing the swelling, and causing a large quantity of pus to flow out. This operation was performed in the presence of Dr. Gross, of Philadelphia, Dr. Neftel, and Dr. Paine. Welch, after conferring with his family physician, one Vaughan, who also held a consultation with two other physicians, J. M. Carnochan and Willard Parker, refused to submit his daughter to further treatment, the latter medical gentlemen claiming that Dr. Sayre had punctured the hip-joint, causing the synovial fluid which lubricates the joint to escape, laming the child for life. Welch, on the strength of this, instituted proceedings, which came up for trial before Judge Jones in the Superior Court; and, on motion by the plaintiff's counsel, the case was referred to William Traphagen, Benjamin Estes, and John Swinburne, M. D., to take testimony. On the trial before these gentlemen it was ascertained that Vaughan was no graduated physician, but had merely been employed in one or two drug-stores; and the testimony of Drs. Carnochan and Parker showed that they had made no microscopic examination of the alleged escaped synovial fluid, which could not be distinguished from serous fluid without the aid of a microscope. The referees reported that they were satisfied that all proper skill and care had been taken in the medical treatment of the patient; that she had not been in the least injured, but much benefited by the operation; and declared the defendant entitled to judgment. This report was confirmed by the Court, which, on motion, granted to the defendant an extra allowance of five per cent. on the amount claimed in the action. The case came before the Court again yesterday, on an order requiring the defendant to show cause why John Swinburne, M. D., one of the referees in the case, should not be removed and another appointed in his place, on the ground of his alleged incompetency. The Court denied the motion, with ten dollars costs.

Mr. Edwin Jones for the plaintiff; P. J. Gage, Ira Shaffer, and J. B. McKean, for the defendant.

APHORISMS ON THE USE OF ARSENIC.—Dr. McCall Anderson, in his lectures introductory to the study of diseases of the skin, now publishing in the *Lancet*, lays down the following aphorisms concerning the use of the great cutaneous tonic, arsenic:

1. Arsenic, judiciously administered, is as safe a medicine as any in the Pharmacopœia, and may often be continued for months without injury to the general health.

2. It often requires to be continued for many weeks, and sometimes the disease seems to resist its action for a considerable time, when, all of a sudden, improvement occurs, followed by a rapid cure.

3. It requires to be given in proportionately larger doses to children than to adults.

4. Infants may be subjected to its influence by administering it to their nurses.

5. The dose should be at first small, and not increased, as a rule, for some time. Then it may be gradually increased till the medicine disagrees, or till the disease begins to yield, when it may as gradually be diminished.

6. It should not be omitted altogether without very good reason, but may be tried in smaller doses or in another form, or omitted for a few days till the bad effects have passed off.

7. Puffiness of the face, or irritation of the eyes, or suchlike physiological effects, if slight in degree, should not lead us to discontinue the medicine; indeed, it is sometimes only then that its beneficial action on the disease is observed.

8. It is decidedly contraindicated in acute cases; and, when its use is followed by marked increase of the irritation of the skin (itching, heat, etc.), the disease is probably not in a state to be benefited by it.

9. It is generally more rapidly effectual if the disease, though in a chronic state, is recent; and the first attacks yield more readily to it than subsequent ones, as a rule.

10. It is contraindicated in most cases which are complicated with digestive derangement.

11. It is apt to produce bronchial catarrh, so that patients should be warned to avoid exposure to cold while taking it; and for this reason it is generally contraindicated in persons laboring under bronchitis.

12. In exceptional cases it may be given with benefit in large doses.

13. It should be given during meals, or immediately after food is taken, for if administered on an empty stomach, it is apt to derange the digestive organs; and it is often better tolerated if given along with a bitter infusion.

14. It should not, as a rule, be entirely discontinued until some weeks have elapsed since the complete disappearance of the eruption.

15. There are few chronic diseases of the skin of constitutional origin—provided they are not syphilitic—which may not be benefited by it (although often other treatment is to be preferred to it), but is especially valuable in psoriasis, pemphigus, lichen ruber, pityriasis rubra, and in many cases of eczema; unless contraindicated as above.

The preparations which Dr. Anderson is most in the habit of using are liquor arsenicalis (Fowler's solution), of which the medium dose for children is two, for adults five, minims thrice daily, and Asiatic pills, of which the following is a modified formula: White arsenic, two grains; black pepper and glycyrrhyza powder, of each half a drachm; with a sufficient quantity of mucilage: divide into thirty-two pills, one to be taken two or three times a day after food.

The fifty-ninth annual meeting of the Rhode Island Medical Society was held in Providence, on the 8th of June. Drs. G. Radeke, C. N. Leonard, Thomas Maher, and A. G. Ham, all of Providence, were appointed Fellows. Prof. Alonzo Clark, M. D., of this city was elected honorary member.

Dr. S. A. Arnold, Secretary of the Trustees of the Fiske Fund, read the annual report of the Board, which was received and ordered on file. No prize was awarded for an essay received during the past year. The Trustees offer the following prizes for the year 1870:

First—Ununited Fractures. The conditions under which they occur, and the most successful method of treatment.

Second—Hydrate of Chloral. Its physiological effects and therapeutical uses.

For the best dissertation on each or either of these subjects, the Trustees will pay the sum of one hundred dollars. Essays must be sent to the Secretary of the Fiske Fund Trustees, Dr. S. A. Arnold, on or before the first day of May, 1871.

The election of officers for the ensuing year resulted, viz.: *President*—Dr. George L. Collins, of Providence; *First Vice-President*—Dr. Lloyd Morton, of Pawtucket; *Second Vice-President*—Dr. Fenner H. Peckham, of Providence; *Recording Secretary*—Dr. Clarence T. Gardner, of Providence; *Corresponding Secretary*—Dr. Charles W. Parsons, of Providence; *Treasurer*—Dr. T. K. Newhall, of Providence. *Board of Censors*—Drs. T. C. Dunn, Newport; Ariel Ballou, Woonsocket; Charles Fabyan, Providence; Otis Bullock, Warren; Sylvanus Clapp, Pawtucket; James Millar, North Providence; J. W. C. Ely, Providence.

CANQUOIN'S PASTE.—At a meeting of the Therapeutical Society, M. Mayet described a new formula for his celebrated caustic, which he had contrived after much investigation. In the new form it retains its consistence for any time, and admits of application in any mode desired. It is as follows: Chloride of zinc, eight parts; oxide of zinc, one; flour, dried at 100° C., seven; and cold water, one. Mix the oxide of zinc and the flour, dissolve the chloride of zinc in the water, and, having added the flour and oxide, rub in a mortar for ten minutes. The paste so prepared in a few hours acquires the requisite consistence, which it henceforth always retains. It may be preserved in a box covered with a layer of starch, but a stoppered bottle is preferable.—*Gazette Médicale, Jan. 8th.*

"EXCELLENT MANURE!"—The special correspondent of the *Times*, writing from Egypt, says: "Such odd things as are happening here! Mutton fattened on ancient Egyptians! It's a fact—a horse chestnut is not a chestnut horse, but, by a sort of *sorites* inverted, we may arrive at the idea of a *gigot*, which shall consist in great part of the dwellers in Memphis. The other day at Sakhara I saw nine camels pacing down from the mummy-pits to the bank of the river laden with nets, in which were femora, tibiæ, and other bony bits of the human form, some two hundred weight in each net on each side of the camel. Among the pits there were people busily engaged in searching out, sifting, and sorting the bones which almost crust the ground. On inquiry, I learned that the cargoes with which the camels were laden would be sent down to Alexandria, and thence be shipped to English manure manufacturers. They make excellent manure, I was told, particularly for swedes and other turnips. The trade is brisk, and has been going on for years, and may go on for many more. It is a strange fate to preserve one's skeleton for thousands of years, in order that there may be fine Southdowns and Cheviots in a distant land! But Egypt is always a place of wonders."—*Medical Times and Gazette.*

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